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	PAGE
POLTERGEISTS: A PHYSICAL THEORY BY G. W. LAMBERT, C.B.	49
RANDOMNESS: THE BACKGROUND, AND SOME NEW INVESTIGATIONS. BY J. FRASER NICOL	71
EXPERIENCE IN A VILLAGE SHOP. REPORTED BY ROSALIND HEYWOOD	87
A CAMBRIDGE APPARITION. REPORTED BY ALAN GAULD	89
REVIEWS 'PHYSICAL AND PSYCHICAL RESEARCH'; 'AN ADVENTURE'; 'INSANITY, ART, AND CULTURE'	91
CORRESPONDENCE DR GELEY'S REPORTS ON THE MEDIUM EVA C. (R. H. THOULESS AND RUDOLF LAMBERT); NOW—THEN! (MALCOLM MACTAGGART)	95
OBITUARY: DR L. P. JACKS BY F. J. M. STRATTON, F.R.S.	100
NEWS AND NOTES	101

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The September issue will include

SOME ASPECTS OF PARANORMAL HEALING. BY LOUIS ROSE,
M.R.C.S., L.R.C.P.

THE ARDACHIE CASE. BY P. J. M. MCEWAN. (A report on occurrences in a country house involving, among other things, rapping phenomena.)

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POLTERGEISTS: A PHYSICAL THEORY

BY G. W. LAMBERT, C.B.

IN the *Encyclopaedia Britannica* (1911 Edition) there is a long article on Poltergeists by Andrew Lang. In the same year (1911) Lang was President of the Society for Psychical Research, and said in the course of his Presidential Address, 'Nevertheless, I do not, at present, believe in a Poltergeist' (*Proc.* 25, 371). Some people, for instance the late Father H. J. Thurston, S. J. (*Ghosts and Poltergeists*: Burns Oates, 1953, p. 152), have been at a loss to know what Lang really thought. Doubtless his position was the same as that of many persons at the present day who, having read the evidence, are unable to dismiss it as all delusion and deceit, but cannot believe that the effects alleged were caused by a tricky spirit.

Since Lang's day no progress has been made in the direction of solving the problem of the causation of poltergeist phenomena. Sacheverell Sitwell, in his fascinating *Poltergeists* (Faber, 1940), ends his examination of the question with the pessimistic conclusion, 'What, by all the powers of good and evil, are these sights and sounds! No one can tell!' (p. 152). Yet he seems never to have abandoned his initial assumption that the phenomena are of human origin, arising from some 'underworld' of the soul (pp. 77-8).

Father Thurston seems to have held somewhat similar views. He found it difficult to believe that the phenomena were due to diabolic agency, because they are so senseless, but thought they had 'their value as a proof of the existence of a world of spiritual agencies, not cognoscible directly by our sense perceptions' (op. cit., pp. 202-3).

In making a fresh attack on this refractory problem we must start from the position that there is no positive evidence that the phenomena in question are due to any kind of *psychic* agency. The fact that no ordinary physical cause suggests itself does not necessarily mean that the cause must be a psychic one. The

attribution of startling events to 'spirits', merely because their causes are unknown, betrays an animistic habit of thought. The word 'poltergeist' implies such agency, and goes back to an age in which men believed in witches and direct diabolic or demonic interference with the ordinary physical course of nature. For want of better, I shall continue to use the word, without committing myself to its implications.

The volume of evidence about poltergeist phenomena is very considerable, and much of it is sufficiently detailed to enable reasonably certain conclusions to be reached as to the *kind* of agency at work. In Appendix I (p. 64) I have listed 54 cases, which I believe to represent a fair sample of the much larger number recorded. They vary in date from the twelfth to the twentieth century, and come from four different continents. The stories have nearly all been chosen from F. Podmore's paper 'Poltergeists' in *Proceedings*, Vol. 12, from Lang's article mentioned above, from Sitwell's *Poltergeists*, and from Thurston's *Ghosts and Poltergeists*. I should like here to acknowledge my indebtedness to these authors, whose industry in making their collections of cases has considerably lightened my task. H. Carrington and N. Fodor, in *The Story of the Poltergeist down the Centuries* (London, Rider, 1953), put the total number of recorded 'unexplained' cases at 330 (p. 83), but many of the cases as listed by them are lacking in detail, especially as to the position of the place affected. In making my list I have chosen cases which can be dated, anyhow as to the year of occurrence, and assigned to a definite small geographical area. It is not always possible to obtain these two particulars, as sometimes there is an intentional vagueness, in order to prevent the house concerned from getting a sinister reputation. In the right-hand column, under 'Primary Effects', I have listed, so far as I can discover them from the narratives, the earliest phenomena observed, before the happenings became distorted and exaggerated by secondary elaboration. The reader will at once be struck by the wearisome monotony.

THE GEOGRAPHICAL PATTERN

The summary at the end of the list discloses the surprising fact that of the 54 cases nearly half are about places within some three miles of tidal water. In Great Britain alone 11 are found in the tidal belt, and only 8 in all the inland area. It is not easy to account for that distribution on the basis of population density.

THE SEASONAL PATTERN

In 33 cases, where I could find the information, I have inserted the month in which the trouble started. In 27 out of the 33 it started in the wet and wintry half of the year. (Lest the reader, in glancing down the column, should mentally note No. 44 as a 'summer' case, I must point out that Wynberg is in the southern hemisphere.) A slight preference is shown for the second winter quarter (15 cases) compared with the first (13 cases).

Looking, then, towards our objective, we want to find a force that is able to tilt a house enough to spill crockery off the kitchen dresser, to make sofas and chairs slide about in the drawing-room, to tilt beds so that the people in them think they are being pushed out of them, to distort windows so that panes are broken (supposedly by stones) and wrench door-frames so that locked doors fly open, and, generally, to strain the timbers of the house in a way that causes them to groan and creak at almost every joint. The force, moreover, must be more often available in winter than in summer, and, comparing one area with another, more likely to show itself near the coast than inland. So far as I am aware, the *only* force which answers to that specification is flood water, and as the water has never been actually seen 'at work', it must be moving in an unsuspected subterranean stream underneath the building that is affected. This is rather a 'bulldozer' of an argument, and, in relation to such extraordinary effects as those to be explained, it is unlikely to carry conviction. We must therefore examine the position in much greater detail.

SUBTERRANEAN RIVERS

At various points along the coast subterranean rivers discharge water into the sea at or below sea level. Their estuaries are normally kept open by scour, just like those of surface rivers, and the tide can penetrate some distance up them. The channel, however, is like a pipe, and if the lower end is for the time being 'blocked' either by a high tide, or, worse still, by sand or shingle washed into the mouth by tidal scour or very rough weather at sea, a spate of fresh water coming down from the land can exert very great hydraulic pressure on the walls of the channel. The pressure can only ease itself upwards, by forcing up the layers overhead. In such conditions Nature provides a sort of powerful hydraulic jack, able to hoist up not a motor car, but a house. Inland the jacking effect only occurs at irregular intervals, especially when the outlet of a subterranean tributary is temporarily blocked by debris washed down the main stream. If the tributary collects water

from a hill, and has a steep fall, its jacking power can be very considerable, when a spate meets an obstruction at the bottom. The spate need not be large, if at the time the water-table is very high, after a long spell of rain. 'It is evident, therefore, that the conditions necessary to produce a maximum flood are a moderate rainfall for a long time followed by a fall of great intensity for a short period of time.' (J. M. Lacey, *Hydrology and Groundwater*: Crosby Lockwood, 1925, p. 75.) These conditions are more likely to be fulfilled towards the end of the winter than at the beginning.

In chalky country intermittent springs usually flow subject to a period of delay after rainfall owing to the time required for the rain water to soak down. Owing to this lag the weather outside at the time of an earth-lift may be quite fine. This causes observers to infer mistakenly that there is no connexion between poltergeists and the weather. For this reason, also, at places where the subsoil is chalk it is difficult to correlate poltergeist phenomena with particular rainy days. Where, on the other hand, the surface is such that the water runs down very quickly, the lag may be only a matter of hours.

THE TIDAL PATTERN

Near the coast, where tides force their way up estuaries twice every twenty-four hours, the hydraulic jacking effect is likely to occur much more often than it does inland. In the coastal case, the effects ought to show some degree of periodicity, corresponding with tidal periods. We must not expect to find an exact correspondence, because, *ex hypothesi*, the jacking effect is the result of two variables, namely flood water and tidal water, meeting one another, the maximum effect being felt when their peak periods happen to coincide.

THE HAUNTED CASTLE IN CALVADOS

In order to test this last point, I have chosen for detailed examination the story of the Château de T., the 'Haunted Castle in Calvados', which, so far as we know from published information, was troubled, on and off, by a poltergeist from 1867 till 1876 (*A.S.P.*, 1892, pp. 211 ff., and *S.*, 268-87).¹ From 13 October 1875 until 29 January 1876 the owner of it, M. de X., kept a journal of the phenomena, and we have enough dates and clock times to attempt to correlate them with contemporary tidal and weather conditions. Unfortunately the exact situation of the Château has

¹ For abbreviations see p. 67.

never been disclosed, but it is to be inferred that it is (or was) not far from the coast of Calvados (Normandy), in the neighbourhood of Bayeux.

The reader, before he is asked to follow a very intricate series of dates and times, may reasonably ask for some assurance that the hypothesis of a subterranean river below the Château is not a ridiculous one, having regard to the character of the terrain in those parts. The following is an extract from the description of the route from Bayeux to the sea at Port-en-Bessin in Calvados taken from Muirhead's *Blue Guide to Normandy* (Macmillan, 1925) p. 85 :

5 miles [from Bayeux] Commes. About $\frac{1}{2}$ m. to the left of the Station is the Fosse de Soucy, a series of natural chasms or 'swallow-holes' in which the Aure, swollen by the Dromine, disappears, to emerge 2 m. away at the foot of the cliffs of Port-en-Bessin.

Where there is one subterranean river, there may be others. We may therefore return with some confidence to the Château. It was built some time after 1835, and was inherited by M. de X. in 1867. From the outset he and his household were troubled by unaccountable noises, which continued on and off till 1870, when they ceased. In 1875, which, be it noted, was a year of exceptional rainfall and devastating floods in Normandy, the trouble started again, and M. de X. decided to keep a Journal, as I have already mentioned. This shows that there were relatively short periods, during which movements of furniture occurred, which I refer to as 'movement periods', interposed between longer periods, during which noises only were heard, referred to as 'noisy periods', or nothing is recorded, referred to as 'silent periods'. The sequence of periods is as follows. **Movement period 13 October**, followed by a noisy period till 19 October, followed by a silent period till 30 October ; then noises till 13 November, when there was a **movement period (13 and 23 November)** ; then a silent period of 25 days till 19 December, when noises started again, followed by a **movement period which lasted from 24 to 29 December**. Noises continued till 5 January 1876, when there was a silent period of 12 days. There were noises again 17 to 24 January, then a **movement period of one day, 25 January**, followed by noises till 29 January, when the Journal was discontinued.

I have put in bold type the times of movement, which constitute 'peak periods', and it will be noticed that they occurred at about monthly intervals, with times of silence intervening as 'valley periods'. It is very curious to find a pattern of this sort in the incidence of such crazy phenomena. In order to test the working

hypothesis it seems worth while to extract the movement periods, and to compare them with tidal and weather conditions at the same times.

Before setting out the detail, some explanatory notes are necessary. First, I have ignored, in reckoning the movement periods, the entry in the Journal against the date 20 December. The complete entry is as follows : 'At a quarter past twelve Mme de X. found on entering her room two chairs placed upside down on two arm chairs. I went into the other rooms. In the blue room I found a chair placed on a side table.' One needs no other explanation than that a servant had turned up the chairs in the way indicated, preparatory to sweeping the floors, and had forgotten to put them in place again. The recording of incidents like this, as though they were pranks of the poltergeist, illustrates the tendency of the human mind to furnish mysterious explanations of quite ordinary events, once the activity of some 'spiritual agency' has been presupposed.

Secondly, in the following comparison the tidal data have been obtained by way of 'dead reckonings' from a 'fixed date', namely 15 November 1875. Early that morning a very high tide met heavy floods coming down the Thames, and 'unprecedented' damage was done in London by the overflowing of the river (*The Times*, 16 November 1875). I have reckoned tides to have been 'high' at fortnightly intervals before and after that date, in the absence of an actual tide table for the period in question.

Thirdly, the weather conditions have been taken from *The Times* meteorological maps and reports, according to the dates on the maps, which at that time showed the ascertained conditions during the preceding twenty-four hours. They usually appeared in the issue of the following day, or, sometimes, of the next day after that. It seems, therefore, that they cover the same twenty-four hour periods as the entries of the same dates in the Journal of M. de X. The maps include the north of France, and where I have mentioned France, or a district of France, it is mentioned in the relevant map or report, and is not a mere inference from weather in England.

The correlations for the 'movement periods' are as follows :

- | | |
|-------------|---|
| 13 October | <i>Tides</i> —moderately high.
<i>Weather</i> —heavy rain on Cotentin peninsula. |
| 13 November | <i>Tides</i> —high.
<i>Weather</i> —rain on Normandy coast. |
| 23 November | <i>Tides</i> —low.
<i>Weather</i> —squally ; rather heavy rain at Dover. |

24-29 December *Tides*—low (24th) to high (29th).

Weather—gale (21st and 22nd); rain in N. France (21st and 27th).

25 January *Tides*—high.

Weather—snow since 13th Jan; thaw began.

The conditions on 13 October and 13 November were obviously very conducive to high pressures in the underground rivers of Calvados. On 14 November there was a bad gale in the Channel, which probably resulted in the blocking of the river mouth with sand. It looks as if the block had remained until the 23rd, preventing a sudden spate of rain water from getting away into the sea on that day. Another gale followed on 21 December, apparently with the same result. There were movements on 24, 25, 26, 27 and 29 December. By the last date the mouth of the river had presumably been scoured out.

On 25 January there was a sudden change in the weather. Early that day the temperature started to rise, and by 6 p.m. had risen several degrees in London. Snow, which had been lying in Normandy since the middle of the month, started to melt. That the thaw had reached the Château de T. is shown by the fact that at 5.10 p.m. that day 'a mass of water' fell down the chimney of the Abbé's study. Before that, however, the torrents from the melting snow must have found their way into the underground rivers. It is not surprising, therefore, that when Madame and her maid went upstairs at 4.30 p.m. they found beds upset, chairs overturned, prints fallen from the walls, and 'everything in disorder'.

From the alarming noises which so terrified the unfortunate inhabitants of the Château it is not easy to draw any certain deductions. Some of the descriptions suggest waves washing up and down a subterranean cavern; others suggest noises made by air imprisoned and compressed by a sudden rise in the river level inside a series of caverns of very unequal height, and then released when the tide went down. Many of the noises, of course, must have been caused by the straining of the timbers of the house.

Enough evidence has now been brought against the tides and the weather to raise against them a very strong presumption of 'guilt'. It is noteworthy that in the Amherst Case also (No. 38) the poltergeist attacks, supposed to be directed against the girl Esther Cox, occurred every twenty-eight days (S., p. 142). It is probably not a mere coincidence that Amherst is on the Bay of Fundy, where the tides run higher than anywhere else in the world.

In addition to the direct action of tidal water forcing its way up estuaries and subterranean rivers, there is another tidal effect of a

more subtle description. The great weight of a rising tide advancing towards the land depresses the shore. 'Now as low tide changes to high tide the position of an enormous mass of water is varied with respect to the land. Accordingly the whole coast line must rock to and fro with the varying tide.' (G. H. Darwin, *The Tides*; John Murray, 1898, p. 122). If underneath the impervious bed which carries the sea, there is a pervious bed sloping upwards inland and fully charged with water, the weight of a high tide must depress the sea floor and pump water back along the whole coast line. 'Wells near the sea frequently flow and ebb with the tide, showing how the sea influences the watertable' (A. Beeby Thompson, *Emergency Water Supplies*: Crosby Lockwood, 1924, p. 15). The effect would be small in the vertical direction, but very powerful in a confined space. It might, in suitable circumstances, cause an earth-lift at points well above sea level. Some such tidal mechanisms, assisted, perhaps, by a flood from the land, might explain the rare cases of disturbance of coffins in vaults, not due to flooding of the vault itself, which puzzled Father Thurston (*T.*, Chap. XIV). He mentions two such 'mystery' cases, one in Barbados and one at Kuresaare on an island in the Baltic. In both cases the vaults are in churchyards quite near the sea. In the former case the geological conditions favour the hypothesis of an underground stream as a causal factor. Thompson (*op. cit.*, p. 29) tells us that below the Barbados coral limestones, which are underlain by tertiary clays, streams of considerable size flow gradually coastwards and at intervals appear as springs on the beach or in the sea. There we have the conditions for the production of tremendous hydraulic pressures, which could heave up large masses of overlying rock, including the vault and its contents. There is a strong presumption that the last two disturbances of the coffins (out of four recorded) were occasioned by flooding below the level of the vault, resulting from hurricanes. I list below five events in order of date. Nos. 1, 3 and 5 are from the particulars furnished by Thurston regarding the state of the Chase vault (*op. cit.*, pp. 158-9), and Nos. 2 and 4 have been inserted by me from information obtained from the *Gentleman's Magazine*.

1. 29 September 1816. Vault opened, coffins found in confusion and put back in position.
2. 21 October 1817. Hurricane in West Indies (*G.M.* 1817, Pt. 2, p. 621). At Barbados some damage on shore and twelve vessels thrown on the beach (*ibid.*, 1818, Pt. 1, p. 75).
3. 7 July 1819. Vault opened, coffins found in much confusion and replaced in order.

4. 13, 14 and 15 October 1819. Hurricane at Barbados 'more dreadful than any since 1780'. Bridges and buildings carried away; some buildings engulfed in the earth (*G.M.* 1819, Pt. 2, p. 556).
5. 18 April 1820. Vault opened, coffins found in great disorder, some upside down.

No hurricane affecting Barbados is recorded in *G.M.* in the period 31 July 1807, when the vault was used for the first time, to 29 September 1816. The disturbances found when the vault was opened on 9 August 1812 and on 29 September 1816 must have been caused by heavy rainstorms not amounting to hurricanes, of which there is no mention in the records available to me.

GEOLOGICAL PATTERNS

The argument that certain geological conditions favour the occurrence of poltergeist phenomena would be considerably strengthened if it could be shown that in some other instances there are reasons for thinking that subterranean water was at work under the 'haunted' house or place.

In 1184 two men living near Pembroke (No. 1) (N)¹ were the victims of a poltergeist. Giraldus Cambrensis, who tells us the story, doubtless knew them both, and heard their description of the happenings. Dirt and other things were thrown about in their houses by an invisible agency which they naturally assumed to be an evil spirit. Attempts at exorcism were a complete failure, a fact which puzzled Giraldus, who concluded that spirits who were out to hoax only, and not to hurt, were not amenable to the disciplinary measures taken against them. One of the men was Stephen Wiriet, who lived at Orielson, about two miles south-west of Pembroke. Descendants of Wiriet lived at Orielson until the middle of the nineteenth century, so the site has a reliable history. It is on a hill of red marls, full of springs, and is surrounded by ponds. The nearest tidal water is in the Pembroke River, one and a half miles to the north, and there is open sea about three miles to the south. There is a limestone stratum at sea level, which appears to extend under the peninsula. Near the cliffs on the south side there is a small lake a little way inland which occasionally 'boils', a sign that air is being blown into it from a subterranean channel. The conditions are much like those in Calvados, and the troubles in the houses of Wiriet and Not were no doubt due to the same causes as those in the Château de T.

¹ (N) signifies that further information about the case will be found under the same serial number in the 'Notes on Cases' in Appendix II (p. 67).

The fact that poltergeist cases in the eastern half of Great Britain are nearly all in estuaries, where tides run strongly, is surely significant. There is the Trinity case (No. 20) just west of Leith, on the Firth of Forth; then Willington Mill (No. 19) in the estuary of the Tyne (N); further south there is the Bishopswearmouth case (No. 21) at the mouth of the Wear. In the Humber area there are two cases, Swanland (No. 24) on the north bank, and Epworth (No. 12), near the tidal head of the Trent, on the south (N). Lastly, in the Thames area there are two cases, the Stockwell case (No. 15) and the Moscow Road, Bayswater, case (see Note on No. 15 in Appendix II). There is a strong presumption that at Stockwell in 1772 the phenomena were caused by flooding of the little river Effra, which runs down from the heights of Clapham through Stockwell to the Thames. In 1923 that river broke out of its channel, and flooded Stockwell station on the City and South London Tube Railway.

Turning now to some inland cases, there is a 'geological group' in the southern counties of England. The Tidworth (or Tedworth) (No. 5) (N), Hinton Ampner (No. 14) (N), and Durweston (No. 45) (N) cases all occurred in valleys of the same description. Between chalk hills lie narrow beds of clay or gravel, along the middle of which run little rivers. At Tidworth the Collingbourne river appears and disappears in a manner that shows that it has a subterranean as well as a surface channel. Mr Mompesson's house is no longer standing, so one cannot go there to listen for the 'Drummer of Tedworth'. There is a strong presumption that it stood over a subterranean tributary bringing flood water down from Sidbury Hill to the main stream. At Durweston the conditions are very similar, with Shilligstone Hill to catch a good head of water to pour into the river. The Ham case, No. 46, on the northern slopes of Inkpen Beacon, probably belongs to the same geological group.

Subterranean water tends to find its way along junctions between different strata. In imagination one can almost see it at work at Hinton Ampner (No. 14). On the 6" Geological Survey Map (Hants 51 N.W.) the line between the upper chalk and the clay runs just past the house, between the house and the stables.

Further, there may be subterranean channels quite near the house, which are much larger than present-day surface drainage requires. H. J. Osborne White, in *The Geology of the Country around Alresford* (Memoir of Geological Survey, H.M.S.O., 1910) suggests (p. 74) that the upper Meon river, which now flows south to the sea, was captured centuries ago by the lower Meon river from the Itchen to which it originally flowed as a tributary in a

northerly direction down the combe behind Hinton Ampner, to join the Itchen at Cheriton. A long spell of wet weather might have raised the water-table sufficiently to cause a heavy flow of water for a time along the old underground channels, which normally carried only a small amount of surface water. The resultant noises might have been very alarming, especially if they were carried up into the house by the shaft of an old well (N).

It is not surprising that houses like Tedworth and Hinton Ampner should be situated as described. Their sites were doubtless chosen as being above flood level, yet low enough to get shelter, and for water to be found at no great depth. But they were unfortunately vulnerable to poltergeist activity.

Another case of geological interest is No. 37, Derrygonnelly (Co. Fermanagh), investigated by Sir William Barrett in 1877. The house, inhabited by a small farmer and his five children, was two miles beyond Derrygonnelly (from the direction of Enniskillen). On Sheet 44 of the 1" Geological Survey Map of Ireland, exactly two miles beyond the place named, there is shown a long junction line, striking across the map in the direction of fall from the mountain to the lough, marking a change from limestone to sandstone. It is ideal terrain for a subterranean stream, subject to sudden spates. Barrett, then a young man in the thirties, five years before the foundation of the Society for Psychical Research, was not in possession of all the information we have today about the pitfalls that beset this kind of investigation. He persuaded himself that he had conversed with the 'ghost' by means of a code of knocks, testing it not only for intelligence, but also for ESP, as we should now call it. With knocks of varying strength, coming at irregular intervals, caused probably by dripping water in a cavern below, it is dangerously easy to 'stop counting' when the 'right' number of knocks has been given. The owner of the house, so far from corroborating Barrett's impression, said he too had tried to get answers to questions by raps, and found that '. . . it tells lies as often as truth, and oftener, I think'. Barrett was the heir to a very long tradition, going back to the early middle ages, that it was possible to 'converse' with 'It' in poltergeist cases. The stories of such conversations are so obviously the outcome of 'secondary elaboration', sometimes invented for the purpose of edification, that one need not regard them as damaging to the working hypothesis we have been considering.

SOME OBJECTIONS ANSWERED

It may be objected that if houses are really jacked up and tilted in the way I have suggested, not only would people have noticed it,

but the houses themselves also would have suffered serious damage, which would have remained as evidence of what had happened.

As to the first point, people have noticed it, but in most cases they have either dismissed the idea as absurd, or else have misinterpreted the experience. The Rev. Samuel Wesley, for instance, relating the disturbances in Epworth Rectory in 1716-17, says, 'I have been thrice pushed by an invisible power, once against the corner of my desk in the study, a second time against the door of the matted chamber, a third time against the frame of my study door as I was going in.' (*S.*, p. 171). His mind rejected the idea that the desk, the door, and the door-frame were moving towards him, and not he towards them, and invented the invisible power to account for lurches for which he knew he was not responsible. In the case at Portland (Oregon, U.S.A.) (No. 48) there is a very illuminating observation by Oliver E. Gidding, a reporter on the *Morning Oregonian*, who was present when phenomena were occurring. In the course of a statement he says :

While this happened and indeed during the course of the several other occurrences I witnessed at the house [546 Marshall Street], there was a creaking and a groaning sound with which the house seemed to be filled.

I have been considerably at sea, and it reminded me of the creaking and groaning to be heard in a gale if one sits in the after cabin of a ship.

I imagined at the time the house was pitching like a ship, but I am convinced that this was imagination, for I did not notice it later while still wondering at the assemblage of sea noises, apart from the crashing it was said was the result of the furniture and utensils coming in contact (*Jnl.A.S.P.R.*, 4, p. 507).

His impression that the house was pitching like a ship was no doubt correct, but his common sense rejected it, when he found that the noises continued after the house had settled down again. His likening of the sounds to the noises made by a ship at sea was peculiarly apposite.

The Portland case is also interesting because the phenomena were observed at five different addresses in the town, which indicates a fairly widespread disturbance. Some damage also seems to have been sustained by the tramlines outside 546 Marshall Street, another indication of an earth movement, presumably caused by underground water (*loc. cit.*, p. 475. For a summary of the evidence see *S.*, pp. 347-53).

As to the second possible objection, it is remarkable how much lifting and straining a house will stand without showing any signs of damage, except, perhaps, to panes of glass and hard wall plaster broken by upward compression. During the second World War

many houses in bombed areas rode harmlessly over shock waves sent through the earth by high explosive bombs, in a manner that astonished their occupants when they went round afterwards to 'inspect the damage'.

SECONDARY EFFECTS

I cannot deal here with all the secondary effects of poltergeist phenomena, in the way of induced hallucinations, fraudulent imitation, grotesque misinterpretations and so forth. There is, however, one very harmful theory that has grown up around them. There is a belief, to which colour was lent by the phenomena associated with the Fox sisters in 1848, that poltergeist activities require the presence of a person, usually an adolescent, who acts as an unconscious medium. In many cases the suspected person, probably a simple servant girl, already frightened by the phenomena, has suddenly become the object of curiosity and distrust. Her actions have been watched, her few belongings have been searched, and she has been accused of deceit. Such treatment has brought on hysterical fits, and has even led to mental derangement and attempted suicide. In the Boston case (No. 32) Mary Carrick, an Irish girl who had only just arrived in America, was the victim. She had hysterics and was taken for a time to an asylum (*S.*, p. 346). In the Milwaukee case (No. 34) Mary Spiegel, a Polish maid, tried to drown herself, but fortunately was rescued (*T.*, p. 121). In the Amherst case (No. 38) Esther Cox was made the subject of the most fantastic imputations (*S.*, p. 378).

In the old-fashioned timber-framed house the subterranean force first affected the kitchen chimney stack, which acted like the concrete base of a seismograph, and then strained the joists and rafters at roof level, making them 'noisy'. These circumstances no doubt tended to fasten suspicion on the maid-servant, who spent her days in the kitchen and nights in the attic. The usual result was the dismissal of the maid without notice. If the hypothesis I have here put forward to account for poltergeist phenomena finds confirmation, it will put an end to the theory of the unconscious medium, and make it less likely in future that similar injustices will be inflicted on harmless individuals.

CONCLUSION

Does the hypothesis advanced in this paper mean that poltergeist phenomena will no longer be of interest to the student of psychical research? No doubt the primary effects, the earth-movements and noises, will have to be handed over to geologists and water

engineers, if they are sufficiently interested to take any notice of them.

The secondary effects, however, will continue to need careful study for their psychological interest. First, there are mistakes of inference, which make observers think that objects dislodged by movements of a building have been thrown by invisible hands, that windows broken by the same means have been broken by stones, that house bells rung, sometimes all together, have been rung by an invisible power of the same kind, in short that a variety of natural movements are due to some mysterious quasi-personal agency. Nature, by misdirecting the attention of observers, can bring off the most remarkably successful conjuring tricks. The impression, for instance, that objects in flight wobble up and down (see e.g. *S.*, p. 71) is undoubtedly due to the *observer* moving up and down with the floor on which he is standing. The apparent wavy movement is no more real than that of the sun moving up and down the sky. The study of human testimony in the face of Nature's sleight of hand may throw some light on the significance of observations made in the presence of other happenings alleged to be mysterious or even miraculous.

Secondly, prolonged fear and mystification can cause otherwise quite normal persons to see hallucinatory persons and animals which are believed in some way to be connected with the phenomena which have induced them. Can these delusive phantoms be distinguished by characteristic features from veridical apparitions of the kind which constitute the central problem in this field of psychical research? Are the emotional states which are conducive to the seeing of the delusive kind of phantom also favourable to the seeing of the other kind? Do the conditions which favour the seeing of delusive images also favour the hearing of voices of the same delusive character as the images?

These questions are posed, not in order to answer them here, but to show that the hypothesis of poltergeist phenomena here put forward leaves many problems for the student of spontaneous cases to solve.

Poltergeist 'outbreaks' still take place from time to time, and are usually reported in the press very soon after their occurrence. The reader of this paper is invited to test the next one he comes across by applying to the evidence the sort of considerations suggested above. To illustrate the kind of testing I have in mind I cite a modern instance, as some people are not impressed by stories, especially ghost stories, of long ago.

In the summer of 1952 a poltergeist outbreak lasting several weeks which was reported in the local papers (e.g. *Widnes Weekly*

News, 3 October 1952), occurred at Runcorn on the tidal Mersey. The *Magazine Echo* (Liverpool) of 28 August 1954, recalling the case gives Sunday, 10 August 1952, as the starting date. According to the theory, to account for the *start* of the outbreak we need (1) suitable rainfall at or very shortly before the time, (2) conditions in the bed of the river likely to cause temporary blocking of outfalls discharging flood water from the adjacent land.

As to (1), *The Times* of Monday, 11 August 1952 tells us that there was heavy rain during that weekend for more than twelve hours in many places, especially in areas bordering on the Irish Sea (p. 3). As to (2), the Chairman of the Manchester Ship Canal Company, in his Address to Shareholders on 28 February 1955, said : 'The Mersey estuary is at best full of vagaries and during the last few years some change has taken place which has aggravated the siltation. It is thought that this may be due to a deep-seated disturbance in the bed of the Irish Sea. However that may be, the fact is that in recent years the incoming tides have deposited much larger quantities of mud in the Mersey Estuary than were normal during the earlier part of the century.'

Further, the Tide Table for Liverpool in *Whitaker's Almanac*, 1952, (p. 174) shows for 9 August a high tide of 31.5 ft., an exceptional height not shown since the preceding February.

The *Guardian* (Warrington) of 3 October 1952 (p. 5) gives a diary of the phenomena from 18 August to 28 September. It discloses three 'movement periods' separated by intervals of about a fortnight each. During that September rainfall was about 50 per cent above normal.

SUMMARY

In order not to obscure the main lines of argument with over-much detail, I have relegated to Appendix II several notes on particular cases. The notes are for the most part confined to points of interest in connexion with the development of the theory put forward, and add appreciably to the weight of circumstantial evidence. I therefore commend them to the reader's notice, and conclude with a brief recapitulation of the main points made in the foregoing paper.

The general grounds for believing that physical forces *initiate* poltergeist phenomena are—

- (1) the geographical distribution of the occurrences which 'favour' coastal regions, especially tidal estuaries ;
- (2) the seasonal distribution of the occurrences, which very markedly 'favour' the winter, compared with the summer ;

(3) the widespread correlations which are found between such occurrences and the meteorological conditions within the preceding twenty-four hours ;

(4) the striking correlations which are found in some cases, where the requisite information is available, between the local geological conditions and those required by the theory ;

(5) the correlations found in some cases near the sea between the incidence of the phenomena and the state of the tides.

All these considerations point to water in movement as the primary agent, a force which can act sometimes silently, and sometimes, in conjunction with air, very noisily, at levels below those open to observation, in such ways that no one knows how the resulting movements are started or whence the noises come. The mechanical effects are so various and occur so suddenly all at once that the observer is deceived, and makes false inferences as to what has happened. He may be deluded into thinking that some of the effects are miraculous or purposive, and that some of the noises are articulate. From that position there is often a regression, under pressure of fear, to primitive beliefs, and even to a state of mind in which hallucinations, coloured by those beliefs, present themselves to his senses.

There may be a few occurrences, classified in the past as poltergeist cases, which do not yield to such an explanation. There are other causes of earth movements than those described, and numerous causes of mysterious noises. The resources of Nature in this field cannot be covered by a single generalization.

APPENDIX I

LIST OF POLTERGEIST CASES

<i>No.</i>	<i>Date</i>	<i>Place</i>	<i>Primary Effects</i>
1.	1184	Orielton (Pembroke)	Dirt etc. 'thrown'.
2.	1654 Nov.	Glenluce (Wigton)	Stones 'thrown', goods broken.
3.	1658	Welton (Northants)	Water, stones and coal thrown.
4.	1662	Plymouth (New Hampshire, U.S.A.)	Stones 'thrown', windows broken.
5.	1662 Apr.	Tedworth (Wilts)	Thumping, 'drumming'.
6.	1670	Keppoch (Glasgow).	Stones 'thrown'.
7.	1679	Newberry (New Hampshire, U.S.A.)	Noises, movements.
8.	1682	Hartford (Conn., U.S.A.)	Stones, earth 'thrown'.
9.	1682 June	Portsmouth (New Hampshire, U.S.A.)	Stones 'thrown', windows broken.

<i>No.</i>	<i>Date</i>	<i>Place</i>	<i>Primary Effects</i>
10.	1682 Nov.	Spraiton (Spreyton, Devon)	Barrel moved, flitches of bacon fell down.
11.	1695 Feb.	Ringcroft (Rerrick, Kirkcudbright)	Peat and stones thrown about in house.
12.	1716 Dec.	Epworth (Lincs)	Groans, knocks, bumps.
13.	1761 Nov.	Bristol	Scratchings, knocks, movements.
14.	1765 Jan.	Hinton Ampner (Hants)	'Footsteps', knocks.
15.	1772 Jan.	Stockwell (London)	China and glass fell, danced.
16.	1806	Slawensick (Silesia)	Shower of lime, heavy blows, movements.
17.	1810	Stampford Peverell (Devon)	Beds shaken.
18.	1818	Munchof (Styria)	'Stone throwing'.
19.	1834 Oct.	Willington Mill (Northumberland)	Heavy 'treads', shaking of windows.
20.	1835 July	Trinity (Edinburgh)	Knocks, 'footsteps', scratching.
21.	1839	Bishopswearmouth (Durham)	Knocks.
22.	1846 Nov.	Rambouillet (France)	Plates 'danced'.
23.	1849 Apr.	Orton (Westmorland)	Knocks, movements.
24.	1849	Swanland (Hull, Yorks)	Small bits of wood flew about, timber rattled.
25.	1850 Mar.	Stratford (Conn., U.S.A.)	Furniture thrown about.
26.	1850 Nov.	Cideville (N. France)	Noises, knocks, hammering.
27.	1853 Jan.	Lipsty (Kharkov, Russia)	Pots on stove moved.
28.	1865 Feb.	Mountfield (Co. Tyrone)	Turf, stones 'thrown'.
29.	1866 (1st qr.)	Tillymoan (Londonderry)	Stones 'thrown', crockery smashed.
30.	1866 (1st qr.)	Larne (Co. Antrim)	Stones 'thrown'.
31.	1866 Feb.	Philadelphia (U.S.A.)	Brushes and combs thrown to floor, ornaments moved.
32.	1868	Boston (Mass., U.S.A.)	Bells rang, raps, chairs upset, crockery broken.
33.	1872	Madras (India)	Stones 'thrown', movements.
34.	1874	Milwaukee (Wis., U.S.A.)	Kitchen furniture moved.
35.	1874 Nov.	Cookstown (Co. Tyrone)	Windows broke 'of themselves', things fell down.
36.	1875 Oct.	Château de T. (Calvados, N. France)	Furniture moved, knocks.
37.	1877	Derrygonnelly (Co. Fermanagh)	Movements, stones 'thrown', raps.

<i>No.</i>	<i>Date</i>	<i>Place</i>	<i>Primary Efforts</i>
38.	1879 Sep.	Amherst (Nova Scotia)	Straw in mattress moved, box jumped.
39.	1883 Feb.	Worksop (Notts)	Table tilted, small objects fell.
40.	1883 Nov.	Wem (Salop)	Saucepan jumped, tea things thrown off table.
41.	1884 Feb.	Arundel (Sussex)	Scratchings, articles fell.
42.	1887 Nov.	Bramford (Suffolk)	Raps, movements of furniture.
43.	1888 Nov.	Resau (Berlin)	Knocks, bangs, movements.
44.	1890 July	Wynberg (S. Africa)	Noises.
45.	1894 Dec.	Durweston (Dorset)	Boots and small objects thrown about.
46.	1895 Jan.	Ham (Hungerford, Wilts)	Furniture moved, objects fell.
47.	1906	Vienna (Austria)	Tools etc. flung about.
48.	1909 Oct.	Portland (Ore., U.S.A.)	Telephone fell off stand, chairs jumped.
49.	1910	Enniscorthy (Co. Wexford)	Tapping, bed moved.
50.	1911 Jan.	Dale (Georgia, U.S.A.)	Trapdoor opened, 'foot-steps', movements.
51.	1919	Coimbra (Portugal)	Windows and shutters opened, 'shrieks', noises.
52.	1921 Feb.	Vieselbach (Weimar, Germany)	Knocks, movements.
53.	1921 Nov.	Liesebrucke (Carinthia)	Movements of small objects, panes broken.
54.	1927 Aug.	Bratislava (Czechoslovakia)	Stones 'thrown', small objects fell.

Places within about 3 miles of tidal water :

Nos. 1, 2, 6, 7, 8, 9, 11, 12, 13, 15, 19, 20, 21, 24, 26, 30, 31,	
32, 33, 36, 38, 41, 42 and 44	- - - - - 24
Inland places, remainder	- - - - - 30
Total	- - - - - 54

Month in which phenomena started noted in 34 cases :

In winter half of year	- - - - - 28
In summer half of year	- - - - - 6
Total	- - - - - 34

Cases in Great Britain :

Tidal places	- - - - - 11
All other places	- - - - - 8
Total	- - - - - 19

APPENDIX II

REFERENCES

In the paper and appendices the following abbreviations are used. In cases where a source is not likely to be readily available to the majority of readers, I have given, when possible, a second reference to a modern source. References to maps are to sheets of the G.S. Maps in the Library of the Geological Museum, South Kensington, London, S.W.

<i>Poltergeists</i> . By Sacheverell Sitwell. London, Faber, 1940.	S.
<i>Ghosts and Poltergeists</i> . By H. J. Thurston, S. J., London, Burns Oates, 1953.	T.
<i>Proceedings of the Society for Psychical Research</i> .	Proc.
<i>Journal of the Society for Psychical Research</i>	Jnl.
<i>Proceedings of the American Society for Psychical Research</i>	Proc.A.S.P.R.
<i>Journal of the American Society for Psychical Research</i>	Jnl.A.S.P.R.
<i>Annales des Sciences Psychiques</i>	A.S.P.
<i>Gentleman's Magazine</i>	G.M.
<i>Annual Register</i>	A.R.

References to Joseph Glanvil's *Saducismus Triumphatus* are to pages in the 1681 Edition.

Cases listed in Appendix I are referred to by their Serial numbers in that list. (N) signifies that further information about the case will be found under the same serial number in the Notes hereunder.

NOTES ON CASES

NO. 1. ORIELTON. 1184. Giraldus Cambrensis, Opera (Rolls Series), 6, 93-4, and T, 7 and 189. The identification of Wiriet's house as Orielson is from Sir Richard Hoare's translation of the *Itinerarium Cambriae* (London, 1806), 1, 202. The walls were probably of dry stones, and there was doubtless much soot in the rafters. Tremors of the kind here envisaged would have scattered much dirt, ejected small stones from the walls, and displaced household utensils.

NO. 3. WELTON. 1658. This place is about 2 miles north east of Daventry. The story is Case 22 in Glanvil's *Saducismus Triumphatus* (pp. 263-8). The evidence is better than in most of Glanvil's 'relations'. The narrative is in a letter from one G. Clark, dated 22 May 1658. It describes a visit he had paid on or about 1 May to a house at Welton, lived in by a widow Cowley, her daughter, widow Stiff, and her two daughters, the younger being 10 years old. Clark found the windows still broken, and a collection of stones which had been picked up, apparently inside the house. The family was a respectable one, not likely to have been victimised by spiteful neighbours. It sounds like a straightforward poltergeist case, if one regards as a secondary elaboration the absurd story, put about by the elder sister, that all the stones, to the number of some 500, and other things thrown about, had been

'vomited' by her younger sister! The child was no doubt sick with fright at the alarming occurrences, which seem mostly to have taken place in the nursery upstairs.

The village of Welton is on the side of a hill, and 'the soil is principally a strong loam . . . well supplied with springs . . . some good limestone . . . excellent brick and tile clay'. (*History, Topography and Directory of Northamptonshire*: F. Whellan and Co., 1874, pp. 439-40). The preceding winter and spring had been very severe and cold (Evelyn's Diary, 17 March and 2 June 1658). The conditions were eminently suitable for a poltergeist effect to develop in wet weather.

No. 5. TEDWORTH (Tidworth). 1662. Glanvil, Pt. II, 89-117, and S. 214-29. This is the story of the famous 'Drummer of Tedworth'. Ella Noyes, in *Salisbury Plain* (Dent, 1913, p. 274) says that Mr John Mompesson's house was on the same site as the present Tidworth House, the official residence of the General Officer Commanding-in-Chief, Southern Command. If that is correct, it is noteworthy that the junction between the upper chalk and the gravel runs very close to the present house, and underneath part of the stables (6" G.S. Map Hants 22 N.E.). In 1662 the force, whatever it was, shook the stables as well as the house, and badly frightened the horses on more than one occasion. Glanvil's own riding horse suffered some injury while he was there on a visit of investigation, and died soon after (S., pp. 222, 223).

The 'drumming' seems to have started in the spring of 1662. At that time the water-table must have been high. Evelyn (15 Jan. 1661-2) records that 'there was a general fast throughout the whole nation . . . to avert God's heavy judgments on this land. There had fallen greete rain without any frost or seasonable cold, . . . in England'. He also records on 17 February 'such a storm of hail, thunder and lightning, as was never seen the like in any man's memorie. . . .'

No. 10. SPRAITON (Spreyton). 1682. R. Bovet, in his *Pandemonium or Devil's Cloyster* (London, 1684), tells this story with a wealth of secondary elaboration, beneath which one can detect a few characteristic poltergeist effects, such as a salt barrel marching about by itself, and flitches of bacon descending from hooks on which they were hanging in the chimney corner. The village, which is about 18 miles west of Exeter, stands high, with steep hills falling away from it on almost every side.

No. 12. EPWORTH. 1716. See *The Epworth Phenomena* by Dudley Wright (London, Rider, 1917), and S., pp. 157-88. The village is on a low hill, which from early times had been much quarried for gypsum. Disused quarries had been filled in, but some of the filling might have been washed out, without visible signs on the surface. The tide comes up the Trent as far as Epworth. There is no convincing evidence of tidal influence on the phenomena as recorded, but their long-continued and noisy character is consistent with such influence. One incident in particular is suggestive. Mrs Wesley observed 5 to 6 p.m. daily as a quiet hour. The poltergeist rudely disturbed it, and Mrs Wesley

begged 'It' to desist. As she had recently offended 'It' by attributing the noises to rats, she had little hope that her request would be granted. Strange to say, 'It' did comply, and from the next afternoon onwards kept quiet between 5 and 6 (S., p. 174). If the noisy period was caused by a particular state of the tide, it would have started about 55 minutes later each day, until a month had elapsed, and Mrs Wesley's request would have seemed to her to have been substantially complied with.

The suggestion that all the mischief was caused by Hetty Wesley (aged 19?) can only be described as ridiculous. She seems to have been just as scared as her sisters (S., p. 184). The fact that her written account (see S., p. 167, Letter XII) is not extant is not evidence that it contained incriminating admissions. It would be more to the point to look for a disused well under the house, with a shaft reaching down to tide level.

NO. 13. BRISTOL. 1761. The phenomena were at the house of one Giles at the Lamb Inn, outside Lawford Gate (T., p. 19). There were two earlier Bristol cases, if we are to believe Bovet, (1) in about 1638, when the house of one Peter Pain in St Mary Poel Street was troubled with noises and movement of a heavy trunk in the long gallery (op. cit., p. 164), and (2) in January 1676, when Mr Meredith's children were attacked with convulsive fits, foaming at the mouth and 'vomiting pins' (p. 167). There is a suspicious resemblance between these two stories taken together and Mr Durbin's story about the fits of the Giles children in 1761-2, during the poltergeist outbreak at that time. It is possible that on all three occasions there were poltergeist phenomena of the kind envisaged in this paper, with secondary accompaniments described in traditionally fantastic terms. It is hardly necessary to point out the strongly tidal character of the Severn estuary.

NO. 14. HINTON AMPNER. 1765. There may, of course, have been noises before the Ricketts took the house in January 1765 (S., p. 241). Our information relates mainly to the year 1771, which was very wet, especially after mid-summer. Mrs Ricketts seems to have left for good, unable to stand it any longer, towards the end of August, during which the rainfall was unusually high (*Metereological Diary* for August 1771 on p. 298 of *G.M.* 1772). I have assumed that the house shown on the 6" G.S. Map is on the same site as that on which the old 'haunted' house stood, which was pulled down soon after the Ricketts left it.

NO. 15. STOCKWELL. The starting date was 6 January 1772 (*G.M.* 1772, p. 41). As already mentioned, 1771 was a wet year, and it ended with a flood in London on 22 December. The brook from Westbourn to Kensington overflowed, and caused serious and widespread flooding in Bayswater (*G.M.* 1771, p. 569). The brooks on the south side must also have been ready to burst out on slight provocation early in January.

Sitwell attributes the Moscow Road, Bayswater, case to the year 1772 also (S., p. 133), but it actually occurred in 1847. Anyhow, it looks as if it was due to the flooding of the Westbourn brook and its tributaries,

which, as the 1771 incident shows, affected quite large areas in Bayswater.

NO. 19. WILLINGTON MILL. The trouble is said to have started three months before 28 January 1835, i.e. in October 1834 (*S.*, p. 190). It is possible that the construction of the viaduct on the Newcastle to North Shields railway (opened in 1840) altered the run of some stream discharging into the Tyne. 'The house stands on a sort of little promontory, round which runs the channel of a watercourse, which appears to fill and empty with the tides' (*S.*, p. 97, note 1). That contemporary description no doubt furnishes a clue to the mystery.

NO. 22. RAMBOUILLET. 1846. This place is between Paris and Chartres. The water-table must have been very high in November of that year. In the preceding month 'owing to incessant rains, widespread and destructive floods occurred in France. The Loire swept away the bridge at Orleans . . .' (*A.R.* 1846, p. 268).

NO. 39. WORKSOP (see *Proc.* 12, 46-58 and *S.*, pp. 145 and 387). The case started on 20 or 21 February 1883. On 12 February great floods prevailed over nearly the whole of England, especially in the midland and southern counties (*A.R.* 1883, Chron. p. 7). On 18 February there was heavy rain, and on 20 February less rain (*The Times* of 23 February 1883, weekly weather report).

The next poltergeist movement was on 1 March. There was no rain for a week before that date, but some fell early that day (*The Times* of 9 March 1883, weekly weather report). The movement at Worksop took place about 11.30 p.m. that night, and was repeated next day.

NO. 40. WEM (see *Proc.* 12, 56-87, and *S.*, pp. 146 and 404). This case started on 31 October or 1 November 1883, and lasted about a fortnight. During the week ended 5 November there were four wet days, and a total rainfall double the average for that week (*The Times* of 10 November, p. 3).

NO. 41. ARUNDEL. 1884 (*Proc.* 12, 67-73). This case started in February. Nos. 39, 40 and 41 probably reflect the wetness of the year 1883, which was exceptional. The tide runs up the Arun past Arundel.

NO. 45. DURWESTON (December 1894 and January 1895) and NO. 46, HAM (January 1895), were doubtless the result of the very wet conditions, including thaws, which obtained during that December and January (*Proc.* 12, 60-95 and 95-101). The Ham case is interesting because it exemplifies two features: (1) two adjacent, but not adjoining, houses both affected to the same extent; (2) in the second house the stool three times, and the chair once, toppled over *each time towards the door*, in full view of one witness, Mr J. Kavanagh (p. 98). This shows that the effects were due to a tilt of the floor so as to make it slope towards the door. Clearly the whole area on which the two houses stood was affected. The efforts made by some observers to fasten the responsibility on to a dwarf girl of 12 were obviously misdirected.

NO. 54. BRATISLAVA. 1927. See *T.*, p. 6. This started as an out-of-doors case, with stones showering on a man and a boy when returning

from fishing in the Tatra mountains. Air, highly compressed by underground water in 'pockets' till forced out through the soil, could cause stones to be ejected with some violence. This 'mechanism' would explain other out-of-doors cases of the kind, including the fact that sometimes the stones are warm to the touch, i.e. on account of the compression. See, e.g., the Sumatra Jungle case of 1903 (*Jnl.* 12, and *S.*, pp. 382-3).

RANDOMNESS: THE BACKGROUND, AND SOME NEW INVESTIGATIONS

BY J. FRASER NICOL

It was, I think, Huxley who said that six monkeys, set to strum unintelligently on typewriters for millions of millions of years, would be bound in time to write all the books in the British Museum. If we examined the last page which a particular monkey had typed, and found that it had chanced, in its blind strumming, to type a Shakespeare sonnet, we should rightly regard the occurrence as a remarkable accident, but if we looked through all the millions of pages the monkeys had turned off in untold millions of years, we might be sure of finding a Shakespeare sonnet somewhere amongst them, the product of the blind play of chance . . .¹

Sir James Jeans, *The Mysterious Universe*.

BELIEF in the reality of paranormal cognition, including telepathy and clairvoyance, rests on four foundations :

- (1) *Spontaneous cases*, such as those collated by Gurney and others (3) and by Mrs Sidgwick (23).
- (2) *Qualitative experiments*, such as those of Guthrie (5, 6), and Miles and Ramsden (13).
- (3) *Mediumistic utterances*, such as those of Mrs Piper reported by Lodge (12) and Hodgson (7), or those of Mrs Leonard reported by, for example, Radclyffe-Hall and Troubridge (18).
- (4) *Quantitative experiments*, begun by Barrett, Gurney, and Myers (1) and by Richet (21) in the 1880's and continuing down to the present day.

The controversy raised by Mr G. Spencer Brown relates to the last of these and does not involve the other three.

Those whose belief in paranormal cognition (PNC for short) is based on the evidence from any two, three or four of the above categories will not be much disturbed by the controversy initiated

¹ On randomness theory the Huxley-Jeans conjectures are logically impeccable. However, it can be shown (but the proof is long) that the time required in which one monkey might be expected to type by chance one Shakespeare sonnet is of the order of 10^{965} years.

by Mr Spencer Brown. Those, however, whose psychic eggs are all in one basket—namely, basket No. 4—may probably have felt a sense of dismay on studying Mr Spencer Brown's views.

Why should this dispute arise after thirty years of almost continuous quantitative research?

The background of the controversy may be described briefly as follows. The major difficulty of qualitative research—that of estimating the chance factor—was overcome in quantitative research by application of the calculus of probability. This was an important step forward.

The second advantage characteristic of all scientific work in which quantitative methods are used is the opportunity they give to create *repeatable experimentation*. By this is meant the designing of an experiment which, found in practice to produce a significant effect, can be repeated by any competent person at any time in the foreseeable future with approximately similar significant results. After thirty years, psychical researchers have failed to produce one repeatable experiment. Yet more than sixteen years have passed since Professor (now Sir Ronald) Fisher made the following statement :

Perhaps I may say, with respect to the use of statements of very long odds, that I have before now criticised their cogency on the grounds, not only that the procedure of calculation is often questionable, but that they are much less relevant to the establishment of the facts of nature than would be a demonstration of *the reliable reproducibility of the phenomena*¹ (3).

The failure of psychical research to meet the fundamental inductive principle of science was bound sooner or later to lead to embarrassing questions. Mr Spencer Brown has now asked them.

As I understand it, Mr Brown has advanced several criticisms relating to the application of probability theory to psychical research data. He has also claimed to have obtained significant results closely resembling those of psychical research by the simple process of comparing sets of digits obtained from a standard table of random numbers.

Why he should have concentrated on these tables is not altogether easy to understand, for if we refer to all the most famous researches concerning straightforward PNC tests (i.e., uncomplicated by other variables such as measures of personality or environment), it is at once apparent that random numbers tables have been used on only a few occasions. Thus in the book

¹ Here and elsewhere in the paper the italics are inserted by the present writer.

Extra-Sensory Perception after Sixty Years (1940), written by five staff members of the Parapsychology Laboratory, Duke University, the authors cite six experiments on which the case for ESP then rested. In none of these researches were random numbers tables employed. In their recent book, *Modern Experiments in Telepathy*, Dr S. G. Soal and Mr F. Bateman bring the evidence up to date. They describe and speak favourably of a number of investigations. In all but a few of these (i.e. the straightforward PNC type referred to above) the experimenters strove to obtain randomness by the old and rather dubious process of card shuffling.

In fact random numbers tables have scarcely any bearing on the validity of PNC claims. What would emerge if Mr Spencer Brown turned his attention from the supposed structure of card-guessing targets to the *actual* target lists on which the main claims for PNC rest, is a tantalising question which is apparently to be left unanswered.

This is not to say that Mr Spencer Brown has been wasting his time pursuing a wild goose into a mare's nest, for in fact the use of random numbers tables has recently become a standard practice in psychical research. These tables provide one half of the data in PNC experiments and are the fundamental material against which the subject has to pit his psychic powers.

In the following pages I shall consider Mr A. T. Oram's recent contribution (16) to this controversy, and, in addition, I shall report some observations on random numbers tables that I have had occasion to collect in the course of my own experimental investigations. But first it is necessary to dwell briefly on the difficult problem of the nature of randomness.

THE NATURE OF RANDOMNESS

The question, 'What is meant by *randomness*?' can only receive the answer, 'We do not know.' Thus :

Random Sequence. A sequence of values that is irregular, non-repetitive or haphazard. A completely satisfactory definition is yet to be discovered.

G. & R. C. James (ed.), *Mathematics Dictionary*.

or again—

It does not seem possible to give a precise definition of what is meant by the word random.

H. Cramér, *Mathematical Methods of Statistics*.

(One recalls, not without feeling, a famous epigram of Bertrand Russell (22), when discussing the axioms of mathematics : 'Mathe-

matics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true.)

But if randomness defies verbal definition we may nevertheless consider the accepted *operational* requirements of a random sequence. For simplicity of discussion, consider an experiment in which only two kinds of cards are used, say black and red, as with playing cards. A random sequence can be obtained from a random digits table by equating the digit 1 to black and 2 to red (ignoring all other digits). From such a table as Fisher and Yates's (2), I obtain the following in fact :

1 1 2 2 1 1 1 1 1 2 2 2 1 1 2 2 2 2 2 1 2 1

and so on. Such numbers are tested for randomness by reference to the following conditions, among others :

1. The numbers of 1's and of 2's should be close to equality. Thus, in 1,000 digits some such result as 505 ones and 495 twos would meet the needs of randomness, but 550 ones and 450 twos would be significantly nonrandom.

2. Pairs or triples of similar digits, e.g., (1 1), (2 2 2), will appear with a certain frequency which can be computed from theoretical considerations. There are three (isolated) pairs and one (isolated) triple in the above set.

3. Other internal patterns, like (1 2), (2 1), (1 2 1) and so forth, will also appear with certain expected frequencies.

Tests of randomness are infinite in number, but the above are among the most commonly applied.

Random arrangement of targets is an inescapable requirement for two reasons :

1. *Statistical.* Randomness is fundamental to the theory of statistical inference. Thus, in the words of two authorities :

It is quite evident that the results of an experiment cannot be supported by probability statements unless the sampling was in fact random. . . . Statistical inference is impossible in nonrandomized experiments.

A. M. Mood, *Introduction to the Theory of Statistics*.

The experiments must be capable of being considered to be a random sample of the population to which the conclusions are to be applied. Neglect of this rule has led to the estimate of the value of statistics which is expressed in the crescendo 'lies, damned lies, statistics'.

'Student,' *Collected Papers*.

'Student' (W. S. Gosset) was discussing the Lanarkshire Milk Experiment and drew the conclusion that failure to randomise the

selection of children as nutritional subjects had invalidated the experiment, which incidentally had cost £7,500 of the taxpayers' money.

2. *Personal.* Subjects in PNC experiments do not make their calls in random order. Rather they tend to repeat characteristic call patterns. Though such patterns do arise in random sequences, subjects call them with excessive frequency. For example, in a recent United States experiment, a university student made sixteen runs through packs of five-symbol cards. In seven of those runs his first two calls were Square and Cross, evidently a private idiosyncrasy of this card-guesser. Still more remarkable as non-random patterning was the following. (The subject did not write down his guesses but had them recorded for him as he called them, by the experimenter (myself) at a table some distance away.) In two successive runs and for the same points in the runs, the subject made the following calls :

	CALL NUMBER									
	16	17	18	19	20	21	22	23	24	25
Run 13	+	+	○	L	≈	≈	△	L	+	L
Run 14	+	+	○	L	△	≈	△	L	+	L

The symbols are, of course, given in the standard 'short-hand' form: + (cross), ○ (circle), L (square), ≈ (wave), △ (star). It will be seen that nine calls out of ten are identical, clearly a non-random patterning effect. If such pairings were commonly encountered in standard tables of random numbers Mr Spencer Brown's theory would be proved true.

Consider a case (19) in which a dowser was invited to guess whether water was running or not running through an underground pipe. The flow was controlled from a tap some distance away and invisible to the dowser. In a series of twenty calls, the target was determined by the flick of a coin, but in a further 50 trials, only every fifth target ('off' or 'on') was determined by the coin, the intervening targets being decided by the experimenter 'mentally', i.e., with no resort to any random process. Two risks were run in this experiment: (1) The dowser's call patterns might coincide with those of the experimenter and produce a spuriously significant high score; (2) Dowser and experimenter might have conflicting call patterns, the combination producing a significantly low score. It was the second eventuality that arose. The reported normal deviate is 3.35, representing supposed odds against the chance hypothesis of 800 to 1, a result which for the reasons given by Mood and 'Student' (above) must be judged spurious—such data cannot be assessed by statistical methods.

KENDALL AND SMITH'S 'TABLES OF
RANDOM SAMPLING NUMBERS'

The tables of 100,000 random digits provided by Professor M. G. Kendall and Mr B. Babington Smith (10) seem never to have been used in British psychical research (judging from published experiments), but they form the subject matter of the inquiry made by Mr A. T. Oram. It happens, however, that I have myself had considerable experience of these tables in psychical work. In the course of the last three years, Dr Betty Humphrey and I, working in collaboration, have used them very extensively, drawing some 66,000 digits from them for transformation digit by digit into the five well-known card symbols: Circle, Cross, Square, Star, Wave. Our research concerned the relationships between the subjects' card-guessing scores and certain measures of their personality characteristics. It is evident that the condition of the card-guessing tests should be as nearly as possible identical for all subjects. In particular it was a *sine qua non* that each subject's target lists *should be random*.

The target lists for all our experiments were based on Kendall and Smith's tables. Since Kendall in the introduction to the tables draws special attention to the fact that certain portions of the table are locally nonrandom, these areas we carefully avoided. Kendall also admonishes: 'Unless there is some good reason to the contrary the tables are to be read across like an ordinary page of print.' Therefore our target lists were based solely on reading *across the rows* of digits rather than down the columns. Then, for reasons that are detailed elsewhere (14, 15) but which may be briefly described as 'experimental rigour', we deemed it advisable to *randomise the order in which the packs were used*. Logically and in terms of the theory of randomness, this procedure is entirely proper. In an ideally random series random selection of batches of 25 digits would result in another series of random numbers. At the end of our experiment, however, I experienced a severe shock on discovering that three out of our 32 subjects were laid at the mercy of significantly nonrandom targets. In the full report of that research (awaiting publication), it will be seen that this non-randomness exerted a damaging effect on the investigation. In this case the effect was not to produce spurious psychical effects but, in the opinion of the experimenters, to suppress genuine ones.

This unfortunate event points to the danger inherent in departing from the directed manner of using random numbers tables. It serves as a reminder that the tables are finite, do not constitute an ideal series, and must be used only in ways that have passed tests of randomness.

Kendall and Smith's tables were tested for randomness *along the rows only*, and Professor Kendall gives the following advice :

Unless there is some good reason to the contrary *the tables are to be read across like an ordinary page of print*. This is the order in which they have been read to be tested. . . . I think it very unlikely that any bias would be introduced if the numbers were read in other ways, e.g., downwards, but *it is as well not to incur the risk*, however slight it may be (10, p. ix).

Disregarding this advice, Mr A. T. Oram in his paper 'An Experiment with Random Numbers' (16) compared *columns* whose randomness properties are unknown.

It might be claimed that while Mr Oram indeed used pairs of columns, yet looked at on a broader view he actually used the rows also. To illustrate the situation, from Kendall and Smith's tables we have the following first 16 digits from the first ten rows (in the original table there are actually 40 entries per row and 25 rows per half-page) :

	1-4	5-8	9-12	13-16
1	23 15	75 48	59 01	83 72
2	05 54	55 50	43 10	53 74
3	14 87	16 03	50 32	40 43
4	38 97	67 49	51 94	05 17
5	97 31	26 17	18 99	75 53
6	11 74	26 93	81 44	33 93
7	43 36	12 88	59 11	01 64
8	93 80	62 04	78 38	26 80
9	49 54	01 31	81 08	42 98
10	36 76	87 26	33 37	94 82

The valid method of comparison in the pseudo-PNC experiment is to equate the digits in pairs of rows. For example, in the first two rows we have : 2 0, 3 5, 1 5, etc. In our table in the first (reduced) pair of rows there are three 'hits' (5 5, 3 3, 7 7). Mr Oram's assistants made the quite different comparisons, e.g. from the first column for Series A, 'contemporary' guesses : 2 3, 0 5, 1 4, etc., and for Series B, 'plus-one' guesses : 2 5, 0 4, 1 8, etc. In the first (reduced) pair of columns there is one 'hit' (1 1). Looked at in another light it might be judged that in Mr Oram's work the rows—known to be random—were used *singly* and that indeed the comparisons were of the form (first row) : 2 3, 1 5, 7 5, etc. This is correct (for 'Series A' *but not for 'Series B'*) but to the best of my knowledge it does not meet Mr Spencer Brown's contention which was concerned (in the present case) with pairs of rows. Mr Oram's procedures do not represent the methods used in practical research and it is difficult to escape the conclusion that

his results are vitiated by the method of using the Kendall-Smith tables.

In the same paper a search was made for 'position effects', and the statistical tests employed were, to the best of my belief, more efficient than some tests that are commonly used. There were two sets of data. Series A comprised 'contemporary hits', like those exemplified first above. Series B comprised 'plus one hits' (comparable with the data of the Shackleton experiment). The data were recorded on sheets in a manner and with a column and row structure very like those of PNC and PK experiments. Mr Oram summed over all the sheets in order to obtain a single position-effect table for each series and a third table for both series combined. Chi-square statistics were applied to test for declines of a vertical form ('down the page') and a horizontal form ('across the page'). Significant results emerging here would be comparable with the decline commonly reported in PNC and PK experiments, the theory in these latter cases being that psi undergoes a slow deterioration of effect as the experiment proceeds.

I have pushed the decline study a stage or two further by using analysis of variance and regression methods, with the results shown below. All the declines are of linear form.

REGRESSION ANALYSIS				
		Series A	Series B	Series A & B together
Columns	t_{11}	1.52	2.23	2.80
	P	.16	.046	.016
Rows	t_{11}	1.71	1.69	2.47
	P	.11	.12	.030

Of these six results three are significant and indicate that something unexpected has happened.

At this point it is necessary to digress somewhat in order to lead up to a comparison of these results with the published records of PK. Recent correspondence in the *Journal*, and information from other quarters, appear to suggest that in the United Kingdom there is a general belief that evidence for the PK hypothesis is based on *over-all scores* from well-conducted experiments that are quite commonly significant. This notion is contrary to the evidence of the published reports, and in authoritative circles in the United States it is fully realized that *the case, if any, for PK rests mainly on position effects*. As an example of how strongly this conviction is held, some three years ago Dr J. B. Rhine in the course of an epistolary controversy (20) with two psychologists of Yale University, stated: 'You did not reply to the main points in my letter: The fact that the main evidence for PK was that of the QD's . . .'

'QD' is an abbreviation for quarter distribution. Fairly early in the preparation of the PK data for publication it was claimed that dice-throwers' scores fell off in the course of the experimental session. The PK scores were recorded in columns spaced across the page, and it was found that when the page was divided into four quarters in the approximate order in which the throws were made (namely : top left—Q1, bottom left—Q2, top right—Q3, bottom right—Q4), there was a tendency for the PK scores to fall off through the four quarters. The so-called 'typical' QD was one in which the scores fell off progressively from quarter to quarter. But it must be remarked that this perfect ideal was only occasionally found in PK data. More often, the discovery was simply that Q1 was greater—sometimes significantly greater—than Q4. The comparison of these two quarters provides the 'QD effect'.

In some pages there was an odd number of columns (or rows), so in order to provide exact quartering, the middle column (or row) was omitted.

We may now deal with Mr Oram's data in the same way.¹ Since there are five rows in his tables, the middle one is omitted. The fairest method of dealing with the quarters would be to compare all four together. This procedure, however, would not give a fair comparison with the practice at Duke which concentrates on Q1 and Q4. At Duke also the statistical method employed is the 'critical ratio of the difference'. Here we shall apply the 2 × 2 table technique which is generally more conservative. The results are as follows :

	SERIES A		
	Q 1	Q 4	
Hits	1025	966	$\chi^2 = 1.94$
Misses	8975	9034	$P = .16$

	SERIES B		
	Q 1	Q 4	
Hits	1027	873	$\chi^2 = 13.85$
Misses	8573	8727	$P = .000,20$

¹ This paper was completed before the publication, in the March issue of this Journal, of Mr Spencer Brown's letter and Mr Oram's reply. It will be seen that the significance of the QD is somewhat more modest than that found by Mr Spencer Brown, the disparity being due to the use of rather different statistical methods. Both methods, however, lead to the same conclusions.

SERIES A & B TOGETHER

	Q 1	Q 4	
Hits	2052	1839	$\chi^2 = 12.94$
Misses	17,548	17,761	$P = .000,32$

Series A is not significant (odds against the chance hypothesis : about 5 to 1) ; Series B is highly significant (odds : 5,000 to 1) ; Series A and B together are highly significant (odds : 3,000 to 1). Series B (or A and B together) provides *the most significant single QD in the annals of psychical research*. The nearest competitor was that found in the PK data of Miss Margaret Pegram whose work dates back to 1934. The 'critical ratio of the difference' in that QD was 3.09 (odds : 500 to 1). In the twenty years that have followed, nothing so striking has been found until now when it is surpassed by ostensibly non-psychic data. Two mutually exclusive explanations appear open to discussion :

(1) The digits in *columns* (as used in the experiment) are not random—a possibility suggested earlier in this paper—and may therefore produce a nonrandom result such as the above.

(2) The digits in the columns *are* random, as assumed by the experimenter, in which event the finding is that the main prop on which the psychokinesis hypothesis rests, is pulled away.

Mr Oram's endeavour to give 'a simple factual reminder that our statistical methods, when tried out in the absence of any possible influence from psi phenomena, do give reliable "chance" results' has not been altogether successful.

FISHER AND YATES'S RANDOM NUMBERS

The first table of random sampling numbers to be printed was that prepared by Mr L. H. C. Tippett and published in 1927 (24). The data were obtained from census reports, and the tables have been quite considerably used in American psychical experiments. Mr G. Udny Yule, F.R.S., has indicated some uneasiness with regard to these tables (25).

The random numbers in the table of Sir Ronald Fisher and Dr Frank Yates (2) were obtained from the 15th to the 19th digits of certain portions of A. J. Thompson's 20-figure logarithm tables. On the first construction of the Fisher and Yates table it was found that the ten digits, 0, 1, . . . 9, were markedly unequal ($P = .075$). This is of some interest in view of some tentative work on derivation of random numbers from Chambers Seven-Figure Logarithms—reported below. Fisher and Yates were apparently dissatisfied with a probability as small as .075, a result produced

mainly by a large excess of sixes. The data were accordingly adjusted by a random process, the sixes for example being reduced to a more acceptable proportion of the whole. The table so modified was published.

There are 15,000 digits spread over six pages. Each page is a square of numbers, 50 per row and 50 per column. The authors state that in experimental work the numbers may be taken from the rows, the columns, or the diagonals. I should like to have examined the table in all three respects, but other occupations forbade so extensive an investigation. The columns, which were analysed, are printed in pairs. Each pair of columns has 50 entries. These I divided in two, so that each double column was in effect two pseudo-PNC runs, the first of a pair of digits being regarded as 'target' and the other as the 'guess'. Identity of 'target' and 'guess' (e.g., 00, 11, etc.) was counted as a hit. In all, the table provided 300 'runs'.

The hits were noted and counted five times, including two forms of special check and an independent count by my colleague, Dr Betty M. Humphrey. The last four of these counts gave the hits consistently as 747. Fisher and Yates give the value as 746. The expected number of hits was 750; for the deviation of minus 3, chi, the normal deviate, is 0.12, which is very close to chance expectation ($P = .90$).

Such a computation is not in itself a refutation of Mr Spencer Brown's theory, since no experimenter is likely to begin his target list with the first tabulated digit and conclude with the last. As Mr Oram has pointed out, the experiment 'might have been designed so as to use only the first half of the table or *some other portion of it*'. In other words, while the final result might be close to chance expectation (as above), significant correspondence might arise at intermediate areas of the table.

To test such possibilities three methods were applied. In the first the score was *accumulated* at the end of each run, the deviation from chance determined, and chi computed. To show how the work was done, data for the first three runs are given below:

RUN	1	2	3
Accumulated Score	4	7	8
Expected Score	2.5	5.0	7.5
Deviation	+1.5	+2.0	+0.5
Standard Deviation	1.5	2.12	2.60
χ	1.0	0.94	0.19

There were 300 of these results, and the largest of the chis was +1.75 at run 19, the associated probability being .08, which in the context of 300 tests can hardly be regarded as of much interest.

Suppose next that the experimenter may start at any point in the table, subject only to the condition that the points be at the beginning of 'runs' (as defined above). A fairly common number of runs in a genuine PNC test is 16. The data were therefore examined in overlapping sequences of 16 runs, i.e., runs 1 to 16, 2 to 17, 3 to 18, . . . 285 to 300; then 286 to 1, 287 to 2, . . . 300 to 15, the whole of the table being considered as of circular form. Taking the critical normal deviate as 2 ($P = .05$), the only significant series obtained were :

RUNS	SCORE	χ	P	RUNS	SCORE	χ	P
6 to 21	53	+2.17	.03	71 to 86	28	-2.0	.05
7 to 22	55	+2.50	.012	73 to 88	28	-2.0	.05
8 to 23	55	+2.50	.012	74 to 89	25	-2.5	.012
9 to 24	52	+2.00	.05	75 to 90	25	-2.5	.012
				76 to 91	28	-2.0	.05

It will be seen that the groups of runs on the left side of the table overlap each other and are therefore not independent; the same qualification applies to the negative groups on the right. The statistical distribution of non-independent results like these is not known to me, but I should suppose that nine significant results out of 300 cases is not a very surprising outcome.

The third procedure called for the *actual* method of table-entry used in psychical research—that is, not restricted to entry at intervals of 25, but commencing at any one of the 7,500 pairs in the table. The ideal method here would be to determine points of entry by a random process; but at this advanced stage of living daily in this veritable ocean of numbers, I already knew with a fair degree of confidence what to expect from such a process.

Instead, I *searched* among my tabulated scores and surveyed the hits (which I had previously inscribed with circles) on Fisher and Yates's pages. No conclusions can be drawn from any effects thus discovered (a well-known statistical rule which in psychical research is sometimes more honoured in the breach than in the observance). I found that the area of most frequent hits was in the early part of the table. Taking 200 pairs (8 runs) from the 261st to the 460th pairs I obtained the following :

Expected Score : 20	Observed Score : 33
χ : 2.946	P : .0032

A search for the largest *negative* deviation over the same number of trials was traced to the pairs 2031 to 2230, the result being :

Expected Score : 20	Observed Score : 9
χ : 2.475	P : .013

(In both cases chi has been corrected for continuity.) These are

the most extreme values available, and it should be observed that, regarding the random table circularly (as before), there are 37.5 independent sequences similar to the one first given above. If the calculated probability is multiplied by this factor, it becomes .12 and hence insignificant. The second probability when so treated becomes .49, close to chance expectation.

Position effects were studied with the following outcomes. The 300 run scores ranged from 0 to 8; the variance of the series was 2.221, which is close to the theoretical variance of 2.250, the probability of the difference being .85. As is usual in quantitative PNC, it was desirable to detect whether our (robot) subject was in better form at some parts of the run than at others. The runs were therefore summed over their 25 trials. An analysis of variance for these internal run positions eventuated in F, with 24 and 120 degrees of freedom, of 1.77, which has probability of roughly .02, and is significant (but see below). A test for decline or other *consistent* variation ended in a chance result. For the QD of my record page, the middle one of the 25 columns was omitted to obtain equality of entries for the four quarters:

Q 1	195	169	Q 3
Q 2	177	183	Q 4

For the comparison of Q 1 and Q 4 we have:

	Q 1	Q 4	
Hits	195	183	$\chi^2 = .426$
Misses	1605	1617	$P = .51$

This result is close to chance expectation.

Apart from the tests carried out on certain hand-picked areas of the data (which were devised in order to favour Mr Spencer Brown's theory) some ten tests were applied or considered. One, dealing with favoured positions in the run, was significant, but in the context of the other null results, its glamour fades, and it is difficult to judge it as better than a chance effect that *ought* to occur occasionally even in ideal random data.

It is open to anyone to apply statistical tests to the rows and to the numbers viewed diagonally (two directions); but in the meantime the Fisher and Yates table can hardly be denied a clean bill of health. This is of some importance, since the table was used in the revolutionary investigations with 'clock' cards of Mr G. W. Fisk and his colleagues (in the early stages at least, and I assume in the later work also). The same table was used by Dr S. G. Soal

and Mr F. Bateman in the highly successful playing cards experiments with Mrs Gloria Stewart as subject.

LOGARITHM TABLES

Randomness and probability have been a fruitful source of misconceptions—though mainly by the laity rather than by the experts. Yet, about half a century ago, so great a mathematician as Henri Poincaré made the categorical pronouncement: 'What is the probability that the fifth decimal of a logarithm taken at random from a table is 9? There is no hesitation in answering that the probability is $1/10$ th.' (17) Apparently the assertion is either quite erroneous, or at the best misleading, for it evidently implies that the probability of a 9 (or any other specified digit) being observed in the fifth (or any other, say the k th) decimal place is *always* $1/10$ th. Professor M. G. Kendall, following Franel, has commented:

Consider the logarithms to base 10 of the natural numbers from 1 onwards. Suppose we choose the k th digit in each and so obtain a series of numbers 0-9. Then the proportional frequency of any digit in this series does *not* tend to a limit as the length of a series increases, whatever k may be. Just what does happen does not appear to be known, but it would seem that certain systematic effects begin to show themselves and these will obviously endanger the randomness of the series (11).

One of the systematic effects may be illustrated as follows. Opening *Chambers Seven-Figure Mathematical Tables* at page 100, I find that the fifth digits of the logarithms 57,000 to 57,010 are:

7 8 9 9 0 1 2 2 3 4 5

Each digit represents an addition of 1 or 0 to its predecessor, and this or similar relationships are characteristic of the entire table comprising some 90,000 numbers. The digits are highly correlated and the series is nonrandom.

I also tried to obtain a random series in the following fashion (which, as readers of our literature will know, is quite unoriginal). I wrote down the seventh digit of every *hundredth* logarithm in the table, i.e., of the numbers 10,000, 10,100, . . . 99,900. Returning to the beginning of the table—10,001, 10,101 and so on. At the end of ten journeys through the table the work had to be terminated for external reasons.

Ten sets of 900 digits each had been collected, 9,000 in all, and it was of some interest to determine whether the digits were equally represented in the sets and also in the grand total. The chi square test for digit frequency applied to each set gave:

SET	1	2	3	4	5	6	7	8	9	10
χ^2	22.1	7.8	9.8	22.1	29.4	11.8	9.8	6.5	9.2	5.6
P	.009	.56	.37	.009	.0006	.23	.35	.68	.42	.79

10 Sets (90 d.f.), $\chi^2 = 134.0$, $P = .0027$
9000 digits, as one group (9 d.f.), $\chi^2 = 17.7$, $P = .038$

In brief, the digits are locally nonrandom in three sets of the ten, the largest chi square having odds against the random hypothesis of 1,600 to 1. The ten chi squares combined have odds of about 370 to 1, and the group of 9,000 digits (not divided by sets) is represented by odds against randomness of 25 to 1. The conclusion is that digits so obtained do not provide a random series.

It will be recalled that Dr Soal and Mrs Goldney obtained their sequence of digits 1 to 5 from Chambers tables in the manner described above, for the Shackleton experiments. However, this was only part of the randomising process, the digits being equated to five cards which were re-shuffled after every set of 50 calls. Though I am not fully informed, the outcome of this double process, according to my understanding, was said to have produced a random series of targets. However, the use of logarithms is evidently not unhazardous.

SUMMARY AND CONCLUSIONS

The foregoing may be summarised as follows :

- (1) Random numbers tables should be used with caution and even with a mild degree of scepticism as to their putative qualities.
- (2) The most significant single QD ever discovered has emerged from comparison of the numbers in Kendall and Smith's tables.

Interpretation of this result remains ambiguous until it is determined either

- (a) that the columns of the Kendall and Smith table do not constitute a random sequence—in which case the significant QD is invalid ; or
- (b) that the columns do in fact produce a random sequence—in which case the highly significant QD is evidence favourable to Mr Spencer Brown's views.
- (3) The Fisher and Yates table, so far as the investigation has gone, is free of disabling nonrandom effects.
- (4) Logarithm tables do not produce sequences of random digits.

The whole matter is apparently one to be decided on the basis of empirical evidence. More of such evidence should be collected.

In conclusion I should like to offer three purely personal opinions on the subject matter of this controversy :

(1) Because of the experimental rigour and the variety of effects produced, it seems most improbable that the Shackleton results will be seriously harmed by any strange pseudo-psychic effects produced from reputedly random digits. But the great mass of evidence in PNC research is of a more modest order, and herein Mr Spencer Brown's inquiries may be of great interest, especially with regard to some of the bizarre position effects sometimes reported.

(2) It seems imprudent to introduce paranormal cognition and psychokinesis into the same argument. The case for PNC is strong, but for PK—well, not so strong. To mingle the two in the same discourse can scarcely fail to do damage to PNC.

(3) Even though Mr Spencer Brown's conjectures become demonstrated truths, there would still remain ample evidence from the qualitative field to sustain a case—I believe a conclusive case—for the reality of paranormal cognition.

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EXPERIENCE IN A VILLAGE SHOP

REPORTED BY ROSALIND HEYWOOD

THE following account of an apparent apparition seems of interest because of its unexpectedness, its normality, and the detached, matter-of-fact attitude of the percipient.

Last autumn Miss Violet Welton, Assistant Warden of St Anne's House, Soho, told me of an apparition seen by her sister, Miss Joan Welton, whose account could be confirmed by a third sister, Miss Beryl Welton. I arranged to meet the sisters, and asked them

to report the occurrence in writing, which, after some persuasion, as they disliked publicity, they kindly agreed to do. They were evidently both very puzzled by the occurrence, saying that Miss Welton had never had such an experience before, and they had never taken any interest in psychic matters.

Their account is as follows :

In the Sussex village in which I lived until two years ago, there was a shop which sold and serviced electrical equipment. It was run by an elderly man, Mr C., his son, and his partner.

One day about a week after Christmas, four years ago, I took an electric fire to the shop to be mended. When I entered there were one or two other people there and the partner was serving them.

Behind the counter, which ran across the back of the shop, there was a short flight of steps leading to the door of a room behind the shop. As I waited, I looked at this doorway and saw the father come out of it and start descending the steps. He looked at me, with the evident intention of coming to serve me. I was rather annoyed to see him, as I particularly wanted to discuss the damage to my fire with one of the younger and more competent men, Mr C. being a great talker and rather slow. To my relief, however, when Mr C. was about halfway down the steps to the shop, his son came out of the back room, walked quickly past his father, without looking at him or speaking to him, and came to ask what he could do for me.

Although I was glad to have the son to serve me, his manner in brushing past his father seemed to me rather rude, and feeling sorry for the old man, I looked to see how he was taking it. He hesitated a moment on the steps and then turned and went up them again, and through the doorway into the back room.

As I saw Mr C. in his shop, he appeared completely normal ; he seemed as solid as anyone else and he looked at me with intelligence and awareness in his eyes, indeed as though he was about to say 'Good morning, what can I do for you?'

I do not think he can have been more than three yards away from me, probably less. Although I went into the shop on many subsequent occasions, I never saw him again.

January 31st, 1955

(Signed) JOAN WELTON

I was cooking in the kitchen when my sister returned from her shopping expedition in the village. I asked her about the electric fire which she had taken for repair to Mr C.'s shop, and who had served her. She told me that the fire had had to be left for adjustment, adding 'I thought I should have to have the old man to serve me, but fortunately the son came forward instead.' Upon hearing this, I dropped my mixing spoon and in amazement gasped, 'But old Mr C. died just before Christmas!' This I knew for a fact, as I had been in the shop about the time of his death, and heard his daughter-in-law saying to another customer that it would cast sadness over their Christmas.

JUNE 1955]

A Cambridge Apparition

My sister was not entirely convinced until we had questioned our maid later in the day, who confirmed the fact of Mr C.'s death the previous year.

January 31st, 1955

(Signed) BERYL WELTON

Miss Welton gave me the following further details in answer to questions I asked on receiving this account. When she saw the father come down the steps, the only other person on the *inner* side of the counter was the partner, who was serving another customer. The light was normal daylight, as the shop was small and the windows large. The old man had on a rather worn dark suit, of the kind in which she was accustomed to see him, and also a pork-pie hat, which she had often seen him wear *in the shop* during his life-time. Miss Welton was emphatic that she could not have mistaken anyone else for the father by reason of dim light, numbers of people, or similarity of appearance. She also emphasized that she had no sense of anything unusual about the figure, and never doubted that it was the old man himself, so much so that she could not bring herself to believe her sister's statement that he was dead until it had been confirmed by the maid.

A CAMBRIDGE APPARITION

REPORTED BY ALAN GAULD

(*Secretary, Cambridge University S.P.R.*)

THE statement reproduced below was written at 3 a.m. on Sunday, 21 November 1954, by an undergraduate of Queens' College within a few minutes of the occurrence of the experience described. For obvious reasons the percipient must be anonymous for the present.

'I have just seen a ghost.

'On leaving the coach in which I had just returned from Oxford, I proceeded to make my way to the [college] gate. I had almost reached the Anchor, when I decided to cross the road. On the other side I clearly saw a dark figure walking slowly towards the bridge. I crossed and as I drew near, a little behind him, I saw what clearly appeared to be an old man, very bent. He held his hands clasped behind his back, and I noticed that he was wearing a morning coat. I was about to step onto the pavement a pace or two behind him, as I had calculated, when quite suddenly the figure disappeared. I was so amazed that I looked all about me and stood several minutes on the bridge before continuing on my way.

'I write this down to prove chiefly to myself that the experience I relate took place in the most normal circumstances, and I am firmly convinced that what I have stated is an accurate description of what I most clearly saw and experienced a few minutes before writing.'

Q 1. Was the figure perfectly normal in appearance?

A 1. Yes. He had the usual button at the back of his frock coat.

Q 2. Did the figure appear aware of your presence?

A 2. No.

Q 3. Did you hear its footsteps?

A 3. On looking back, I didn't ; at the time I didn't notice.

Q 4. Had you recently taken any alcohol?

A 4. I had had a small glass of whisky before leaving Oxford, but that was three hours previously, and I had slept in the coach.

Q 5. How long was the figure in view?

A 5. A minute to a minute and a half.

Q 6. What were the lighting conditions at the time?

A 6. It was a clear and starlit night. I could see clearly over the bridge to the bend in the road. I can't remember whether the street lamps were on.

[It has since been ascertained that the main lamp some 50 yards up Silver Street would have been alight.—A.G.]

Q 7. Have you heard any rumours of a ghost in Silver Street?

A 7. Never. I had remarked some time before this happened that Cambridge had no ghosts, and that this was surprising for such an old town.

Q 8. What was your attitude to the subject before you had this experience?

A 8. Sceptical. I had no interest in Psychical Research.

The percipient added that the apparition disappeared just beside a bricked-up window in Queens' College. He was returning late from a hockey match, and was watching it carefully in order to avoid being recognised when about to climb into College. The time between his seeing the figure and writing his statement was about ten minutes.

[Although there is no evidence of its being veridical, this experience is notable for having been recorded so promptly. It is, indeed, an object lesson in this respect. Professor F. J. M. Stratton, of Gonville and Caius College, Cambridge, who was President of the Society for 1953-5, reports that on asking about

the figure's headwear he was told that it 'wore no hat, was baldish and had silvery hair, which was long and curled up slightly at the bottom.' He adds that the scene was Silver Street, on the East side of the River Cam, and that there was no moon that night. The percipient has very kindly presented his original signed statement to the Society for its files.—ED.]

REVIEWS

PHYSICAL AND PSYCHICAL RESEARCH: AN ANALYSIS OF BELIEF.

By C. C. L. Gregory and Anita Kohsen. Reigate, Omega Press, 1954. ix, 213 pp. 15s.

One of the great problems in psychical research is the disparity of belief between persons who have had apparently identical experiences. One may believe passionately in psychical phenomena and call all those who disbelieve cowardly sceptics, and another may disbelieve equally passionately and call the believers fools and possibly knaves. Nor is this situation confined to psychical research; the study of cosmology has been said to be a subject where 'emotion takes the place of fact'. It is therefore always welcome when someone has the courage to write on this difficult topic. Gregory and Kohsen do so from a stimulating standpoint.

In the first chapter, the authors define and discuss four functions which are commonly used in deriving knowledge: (1) logico-deductive synthesis, (2) inductive synthesis, (3) the operational method, and (4) intuiting. They then proceed to discuss the extent to which each of these functions is used in modern scientific thought. This is done with the aid of a large number of examples. These examples are brought into the argument with little relation to each other and the net result is a considerable confusion in the mind of the reader.

This is broken by a long chapter on the theory of relativity, which is discussed in detail. This reviewer, a teacher and researcher in physics, was particularly disappointed by this chapter. The exposition is such that it is doubtful whether anyone not already well acquainted with the subject will be able to understand the meaning. It seems to be a large part of the authors' case that in the derivation of this theory inductive synthesis played only a small part, and they quote Einstein to show that it was intuitively conceived; if they go further they surely err; the conception of a theory is not the same as its acceptance. There are many theories which were brilliantly conceived, but

which are now forgotten by all but the scientific historian. The inductive system and the operational method were essential for the detailed verification of the theory, without which it would not have had universal acceptance. Popular tracts on relativity, intent on exposition of facts rather than on the method of deriving them, often ignore the verification of the theory: it is a pity that the authors appear to do so also.

The fourth chapter on psycho-analysis is much better written than the rest of the book. Unfortunately this reviewer has not the ability to discuss it in detail but he obtained a distinct impression that psycho-analysis is as yet still very unscientific.

The purpose of the first four chapters is to lead up to the fifth—that on psychical research. Here the authors seem to advocate a relaxation of the criteria for belief. It is somewhat obscure why this relaxation should be in one direction rather than another; certainly, if there is no better knowledge the only way to make any progress is to accept something as a working hypothesis, and maybe to allow that hypothesis to direct one's everyday actions. This is the solution of the Spiritualist. But the authors go further and appear to deny the validity of attempting to verify the working hypothesis in the same way as the theory of relativity has been verified.

In the course of this argument they stress the urgency of the problem. Here they state, 'Science as a discipline is magnificent and indispensable; as a belief system it is disastrous'. The implication is that a scientific system of belief is general and is perhaps a cause of social troubles. But surely a scientific belief system is not held by more than one per cent of the population? The rest use, without thought, the technical products that were evolved by scientific method. Science as a belief system has not been tried by the community as a whole.

This book will certainly stimulate thought; I fear, however, that it is too stodgy to stimulate clear thought.

R. WILSON

AN ADVENTURE. By C. A. E. Moberly and E. F. Jourdain.

Fifth edition. Edited by Joan Evans. London, Faber, 1955.

131 pp. 6 plates, 4 maps. 12s. 6d.

The main interest of the fifth edition of this famous book lies in the character sketch of the two authors contributed by the editor, who knew both of them, particularly Miss Jourdain, well. In 1901, when they had their experience at Versailles, the acquaintance between Miss Moberly and Miss Jourdain was comparatively slight, and Miss Jourdain, the junior by nearly twenty years,

was very much under her senior's thumb. Miss Moberly 'could read, but would not try to speak, French'; Miss Jourdain wrote and spoke the language well, but until the year before her experience had never got as far as Paris. These limitations must be borne in mind in assessing their ability to distinguish between the French scene of 1901 and of 1792. 'Neither of them had . . . in 1901, any experience in historical research. . . . Neither had had any experience of experimental psychical research and they never at any time engaged in it' (pp. 18, 19).

The crux of the whole business is, of course, the existence of two very different accounts of their experience by each of the authors, called in my note on the evidence in the *Journal* (35, 178) M1 and M2, J1 and J2, and the uncertain date of M2 and J2, now existing only in copies made in 1906. Their statement, repeated by Dr Evans on p. 19, that they were drawn up in November and December 1901, a few days or weeks after M1 and J1, is for the reasons I gave in my note hardly credible. Dr Evans is at pains to point out (p. 20 and footnotes to later pages) that the divergences between the printed versions of M2 and J2 and the 1906 manuscript copies of them are insignificant, but the reader should not assume that the same is true of the discrepancies between M1 and M2, J1 and J2, which are all that matter. She ends her interesting Preface: 'The reader must make up his own mind. I hope that I may have simplified his task by the production of this edition.' She would have simplified his task still more by setting out M1 and M2, J1 and J2 in parallel columns. No edition has so far done this, and in all five editions M1 and J1 are suppressed entirely, except in the second (1913) where they are relegated to an Appendix.

One is glad to note an appreciative reference to Mr G. W. Lambert's articles in the *Journal* on Antoine Richard's garden.

W. H. SALTER

INSANITY, ART, AND CULTURE. By Francis Reitman, M.D., D.P.M. Bristol, John Wright, 1954. 111 pp. 6 plates. 12s. 6d.

The author extends the studies described in his previous book *Psychotic Art* (reviewed in the *Journal* for May-June 1951) and compares the influences of cultural factors and of mental illnesses upon the qualities of pictorial productions. Of particular interest to us is his chapter entitled 'Psychic Art'. In this he compares the 'automatic' pictures that spiritualists and mediums produce when supposedly under outside guidance with the pictures produced by patients in mental hospitals. He finds that certain features occur in

both psychic and psychotic pictures—notably ‘bizarreries’, irrelevancies, over-elaboration, peculiar labelling, and the use of symbols. While he points out that the commencement of automatic painting is sometimes the prelude to a frank psychotic breakdown, Dr Reitman does not consider automatic painting to be invariably a sign of psychosis. The features found in psychic and psychotic paintings also appear sometimes in the paintings of eccentric persons who are not mentally ill, and also in the paintings of normal persons in special circumstances, e.g. doodling and ‘free-painting’ classes. Dr Reitman wisely draws no conclusions.

D. J. WEST

ARCHIV ZUR KLÄRUNG DER WUNSCHELRUTENFRAGE. 1954, No. 1.

The present issue of this journal, which is edited by Count von Klinckowstroem and Freiherr von Maltzahn, contains, apart from book reviews, only articles by the editors. The first article is devoted to the listing of testimonials on dowsing by various learned authorities. As is to be expected, most of these testimonials are favourable and the author of the article, von Klinckowstroem, may have felt that a long array of impressive sounding titles such as ‘Wirklich Geheimer Admiralitätsrat’, ‘Geheimer Bergrat’ etc. may persuade by virtue of authority otherwise sceptical scientists to accept the reality of the phenomena. I suspect that many scientists may, like the reviewer, prefer controlled experiments to prestige suggestion.

Most of the other articles in this issue attempt to list striking individual successes by dowzers. For example von Maltzahn, by quoting some of his own successes, suggests that they could not, as is often alleged for dowsing work, be the results of geological clues. However, absence of adequate scientific controls exposes the author to the argument that his results could have been fortuitous. Like the best-known case histories of spontaneous phenomena in parapsychology, these dowsing successes may at most be taken as suggesting a phenomenon, but they cannot be claimed as evidence which is based on the scientific method.

Even an article on Mortillet who was both an anthropologist and a dowser does not, in spite of its cultural interest, carry any more conviction. This is not to say that the phenomena are not possibly genuine, but to suggest that it would be desirable to make a start on *controlled* experiments in this field rather than to fill volumes with personal anecdotes and hearsay which are of little scientific interest.

G. D. WASSERMANN

CORRESPONDENCE

DR GELEY'S REPORTS ON THE MEDIUM EVA C.

SIR,—In the November-December number of the Journal there is an article by Rudolf Lambert in which it is stated that Geley suppressed evidence of fraud by Eva C., that Osty consented to that suppression, and that Herr Lambert himself knew of the suppression but was unable to say anything about it because he was only told about it in return for a promise of secrecy. The article is followed by an editorial footnote saying that Lambert has also made criticisms of the Martin-Stribic ESP experiments which may be seen in the Society's library. Both article and footnote seem to raise disturbing questions on standards of criticism in psychical research.

The reference to the Martin-Stribic experiments in the footnote seems to me very improper. The criticisms should either be published or not referred to. Since in fact readers of the article will not generally consult the unprinted article in the library, this reference to unstated criticisms can only lead to the general impression that there is something fishy about the Martin-Stribic experiments but it is not known what. It should be a concern of the S.P.R. to get rid of such vague defamation and to replace it by definite criticism which can be answered if it is unfounded and from which other workers can profit if it is well founded.

The article itself also raises misgivings. If the facts are as reported, they should certainly be known. There is, however, no supporting evidence of any kind. One of the generally agreed conclusions of the study of psychical research is the unreliability of human testimony. Do we suppose that this unreliability only applies to the reporting of paranormal phenomena and not also to the reporting of the behaviour and statements of other psychical researchers? The question is not as to the truth of Herr Lambert's statements but as to their evidential value. Are we to adopt a wholly different standard of evidence with respect to statements about the character of a psychical researcher from that we demand for statements as to paranormal phenomena?

It appears that Herr Lambert has been silent on this matter for 25 years because the facts were only revealed to him in return for a pledge of secrecy. It is not the first time I have heard of something dubious in psychical research having been revealed in return for a promise of secrecy. I would suggest that psychical researchers should make a rule for themselves never to give such a promise. It has both the disadvantage of making the person giving the promise an unwilling accessory to concealment or fraud and also of having

the result that when the revelation is ultimately made, it has no evidence in its support except the bare word of the person making it. Furthermore, those against whom the charges are made are no longer alive and able to give their own version of the facts reported.

R. H. THOULESS

Cambridge.

SIR,—I thank Dr Thouless for his courtesy in permitting me to read his letter addressed to you.

Dr Thouless says about my report on the Geley photographs: 'If the facts are as reported, they should be known. There is, however, no supporting evidence of any kind.' I agree with Dr Thouless that because of the unreliability of human testimony this might seem a grave objection. But, as I wrote in my report, it was Count Perovsky, former Honorary Member of the S.P.R., who in Paris raised my suspicions against Eva C., so that I twice discussed this matter with Dr Osty, who in the Institut Métapsychique showed me the photographs left by Geley. Immediately after my visit to the Institut I dined with Perovsky who knew that I came from Osty; Perovsky saw that I was deeply disturbed and disgusted by what I had heard and seen, and though I could not disclose him all, the little I said added to his knowledge of this shocking case. All this I wrote in my original report dated September 15th, 1952. The information Perovsky had was published in 1928 by Count Carl von Klinckowstroem (now Corresponding Member of the S.P.R.) in the third Vol. p. 113 of the *Zeitschrift für kritischen Okkultismus*. Perovsky had also communicated to Klinckowstroem the names of the persons to whom he owed his knowledge, demanding, however, that he should not publish these names. Klinckowstroem therefore indicated these persons by the letters A, B, C. I may say that C. stands for myself. Mr A. (Père Mainage) spoke of evidence involving Madame Bisson; I admit that the stereoscopic photographs I saw make it very possible that Madame Bisson helped in the fastening of the 'materialisations' in Eva's hair, but with an observer as incapable as I believe Geley to have been such a help was perhaps not necessary. His incapacity is proved by his declarations about Eva's hand-control; every reader of my report can compare my remarks about this with the corresponding pictures in the original French edition of *L'Ectoplasmie et la Clairvoyance* (or in the German and probably also the English translation, where the sequence and numbering of the illustrations is surely the same).

Klinckowstroem further publishes part of a letter written to him

by the French writer Paul Heuzé (pp. 112-13 of his article). Heuzé says that an eminent French parapsychologist had told him in July 1927: 'We have now in our hands the absolute proof (by stereoscopic photographs) that Eva C. has always shamelessly frauded . . .'. Like Klinckowstroem I do not doubt that Heuzé's 'parapsychologist' was Dr Osty himself. So my testimony does not stand alone.

In the *Zeitschrift für Parapsychologie* 1928, p. 299, Schrenck-Notzing published a letter from Professor Richet where Richet admits that his 'friend Osty' told him that he had found negatives of photographs (taken by Geley and Mme Bisson) that seemed to indicate fraud. Richet adds that as these photographs had already been published by Schrenck and Mme Bisson (and Geley), publication of these findings by Osty would bring nothing new and would therefore be useless. But the mediocre reproductions of these photographs published by Geley, Schrenck-Notzing and Mme Bisson do not produce at all as damaging an impression as would be made on every serious person by Geley's original stereoscopic negatives; and therefore there remains the sad question I raised, why Geley did not say one word about this deplorable character of his negatives.

R. LAMBERT

Stuttgart-Degerloch,
Germany.

[Is not Dr Thouless straining his language in applying the word 'defamation' to Herr Lambert's expression of opinion that the Martin-Stribic experiments are open to criticism in the light of what he considers the proper conditions to make experiments fraud-proof? Those who read Herr Lambert's paper in full can form their own opinion as to whether he makes out his case. Those who do not are not likely to be much influenced by a brief note of this kind.

As to the Geley photographs, Mr W. H. Salter, who was present at the Paris Congress of 1927, writes:

It would have been better if Richet and Osty had at the time published a statement on a matter which I heard freely discussed there; each could have told us plainly why he did or did not regard the photographs as adding to our knowledge of Eva C.'s phenomena. Osty, who was in a difficult position, took what was perhaps the next best course of confiding in Herr Lambert, in whose fairness and discretion he very properly trusted, and who stood outside the internal disputes of the Paris Institute.

In a letter to me our Corresponding Member Mr Carl Vett, who

organized the Congress, says he is convinced that Herr Lambert is wrong as to the reason why Mme Bisson was not at the Congress; her absence, he says, was due to illness.

On the simple question of whether Herr Lambert was shown certain photographs by Osty and given a statement as to their origin and the reason why they had not been published, there seems no good reason for doubting Herr Lambert's word.—ED.]

Now—Then!

SIR,—It is already the standard practice among those who attempt to discuss such matters to distinguish terminologically between pre-cognition and post-cognition. The purpose of this letter is to submit that a further term is needed—namely, *then*-cognition (not-now cognition). *Then*-cognition would be held to be present in instances of statistical experiment when the *difference* between a present significant degree of pre-cognition and a present significant degree of post-cognition was not itself sufficient to constitute a significant deviation. Given that E and O constitute respectively any degrees of significant pre-cognitive and post-cognitive deviation, *then*-cognition would be said to be present to the exclusion of both E and O immediately the difference between E and O, no matter how impressive for either when reckoned apart from the other, itself fell below a level of deviation acceptable as significant. If in a series of, say, 768 trials, there were to be concurrently, say, 194 pre-cognitive and 195 post-cognitive hits, then, although this would constitute 3.65 and 3.74 Standard Deviations respectively *when reckoned separately*, the difference between the two figures would be itself so slight as to rule out all possibility of asserting a significant result as seen between the pre-cognitive and the post-cognitive identifications.

I do not suggest such a terminological distinction in any sense either idly or gratuitously; there *may* be here something of the first importance for our better understanding of the position currently reached by statistically conducted paranormal research. The point which primarily underlies my indication is already far from being new; it is excellently made by E. A. Burtt in an article entitled 'Real and Abstract Evolution', which was published in the *Proceedings* of the Sixth International Congress of Philosophy. He says:

... the world as empirically revealed always begins in the present, and remains within it while expanding into the past and the future. This may sound startlingly paradoxical—the opposing view would, however, be much more startling if it were not so fully ingrained in our

thought-habits that we never dream of questioning it. . . . Appeal to fact on such a matter may be unconventional and embarrassing, yet I beg of you to consider whether the world as actually revealed to any of you began in a remote past with Space-Time or electrons, or whether these things did not emerge after the world had gone through many adventures and assumed many shapes. . . . Real evolution, that is, evolution as empirically discovered, is not a movement from past through present to future (such a process is itself an emergent abstraction from the course of real evolution), it is evolution from the present into both past and future. The world always takes shape from the present outwards.

I have often read these words ; what I get from them is that it is explicitly from the moment of current attention (or *now*) that we have to seek the consistent extension of our ideas ; the set-up of *before and after* (identical with the causal ordering of things ever since causality was cut aside from its assumed necessity by Hume) is to be imported into the set-up of *past and future* only as an explicitly recognized abstraction, and about *before and after* there is *per se* nothing to place it as past any more than as future. In fact, for me, it comes to this : *now* and *attention* are simply contextual variants of the same thing, and what is not now is simply *then*—i.e., indiscriminately both past and future. This indiscriminateness is what I also mean when I speak of *anticipation*. From the point of view of my own ingrained habit of language the point is one that could scarcely be more difficult for me to make—obviously, so it seems, I anticipate what is going to happen—what is in my immediate future. But from the empirical point of view my anticipation cannot be like that ; here it proceeds merely presentwards by a relation that is indifferent to the difference between past and future. I anticipate purely what is not now. There has been, for me, even worse than this ; there seems (when once the language control begins to be slackened) every reason for supposing that the attention-anticipation difference is to be identified with the difference holding between being asleep and being awake. Why? Because (a) of the fact that surprise is persistently absent from our dreams. When we are able to question in a dream what is going on, we are upon the point of waking up. And surprise is simply anything that is contrary to anticipation. Attention, on the other hand, is our grounding against surprise. And because (b) we dream post-cognitively and pre-cognitively in just about equal amounts. . . .

The material I here submit is 'rough-hewn' : there can be little point, in the space of a letter, in trying to 'polish' it. For me it affords the speculation that it is by some inclusion of our sleeping or attentive state that *then*-cognition is to be construed as possible.

This leaves me having to ask : what is the nature of the *pre* and the *post* of cognition that is distinct from *then*-cognition? But that is a further question. . . .

MALCOLM MACTAGGART

Welwyn, Herts

OBITUARY

DR L. P. JACKS

By the death on 17 February of Dr Jacks at the age of ninety-four the Society has lost its oldest Vice-President, though happily not its oldest member. Dr Jacks joined the Society in 1909, served for some years on its Council, and was President for the years 1917-18. His Presidential Address on 'The Theory of Survival in the Light of its Context' reflects one of the great interests of his life. In the last of his many books, *Near the Brink*, published after his ninetieth birthday, he returned to the idea of reciprocity in death : 'We think of the dead man as leaving us ; maybe he thinks of us as leaving him.' The title of his book *All Men are Ghosts* reflects again his thought-provoking attitude towards many of life's problems, an approach from the opposite side to the normal one.

Dramatic dreams were of considerable interest to Dr Jacks and were the subject of a lively correspondence in our *Journal*. He attended sittings with Mrs Leonard and was impressed by the dramatic quality of some of the communications that he received, purporting to come from a young friend killed in an air-fight. With Dr Schiller he unsuccessfully tried to investigate what he described as a remarkably well-attested ghost, but though he never saw a ghost he claimed to have *smelt* one. In *The Confessions of an Octogenarian* he mentions that one night, when he had been engaged in writing a difficult chapter of *The Life of Stopford Brooke*, 'quite suddenly the room became filled, almost overwhelmingly, with the unmistakable odour of Manila cigars. It lasted some minutes and then vanished as suddenly as it came'. Stopford Brooke was a great smoker of Manila cigars and his study was always saturated with their odour. Most unlikely explanations were pressed upon him by friends ; his comment was, 'Fools have been divided into two classes : those who believe everybody ; and those who believe nobody. The latter strike me as the more foolish.'

A courageous and independent thinker, a stimulating writer, Dr Jacks had a large following in the United States. A distinguished Unitarian minister, Professor of Philosophy and later

Principal of Manchester College, Oxford, he was best known to the outside world as editor of the *Hibbert Journal* from its foundation in 1902 to 1947. The *Journal* was open to all schools of thought in theology, philosophy, and religion, and Dr Jacks, in maintaining its catholicity of outlook, threw its columns open to psychical research on a number of occasions. In particular Sir Oliver Lodge, an old friend from Liverpool and Birmingham days, was one of the band of distinguished contributors who helped the editor to secure for his *Journal* a sure place among the national quarterlies.

F. J. M. STRATTON

NEWS AND NOTES

The Society's new President

Mr G. W. Lambert, C.B., has been elected President for the year 1955-6. A member of the Council since 1925, Mr Lambert has made a special study of spontaneous cases. He has contributed several papers to the *Proceedings* and *Journal*, including, in recent years, 'The Dieppe Raid Case' (*Journal*, 36, 607-18), an investigation, with Mrs C. H. Gay, of a case of apparent retrocognition, and 'Antoine Richard's Garden' (*Journal*, 37, 117-54, 266-79; 38, 12-18), a re-interpretation of the evidence in *An Adventure*. His latest contribution appears in the present issue.

Cambridge Conference on Spontaneous Cases

At the International Conference of Parapsychological Studies held at Utrecht in 1953 it was recommended that the S.P.R. should organize a smaller conference to discuss spontaneous cases. This has now been arranged for July next when about twenty-five delegates from the United Kingdom, the United States, Denmark, France, Haiti, Italy, the Netherlands, Norway, and Switzerland, will meet at Newnham College, Cambridge, a place which has a very close and long-standing connection with psychical research. The Conference is receiving the generous support of the Parapsychology Foundation of New York.

On the afternoon of Monday, 11 July, the delegates will be received by the President and Mrs Lambert. On each of the four following days it is planned to have sessions to discuss the contribution to be made to psychical research through spontaneous cases (this matter being introduced by Professor Gardner Murphy), the various types of spontaneous case, including apparitions of the living and the dead, 'travelling clairvoyance', haunts and poltergeists, and especially the best methods of collecting and

investigating cases of these types and of making known to the public such instances as may be of sufficient interest. Particular attention will be invited to the psychology of this type of material and the use of experimental methods in investigating it.

The Conference will end on 16 July and the delegates will disperse on the 17th. It will be held in private, and will not be open to the public or the press.

ESP in Spain

Dr S. G. Soal and Mr F. Bateman visited Spain in April to take part in the investigation of a promising Spanish ESP subject, Maria, a fifteen-year-old nursemaid in the household of Mr John Langdon-Davies. Clairvoyance tests were first carried out, in good conditions, but 1,200 guesses gave only chance results. Telepathy trials followed, in which the agent was mainly Mr Bateman, and 4,200 trials yielded an excess of 100 hits over chance expectation, corresponding to odds against chance of 8,000 to 1. The investigation is being pursued.

An article in the *Sunday Pictorial* for 8 May gave an account of these experiments. In this the figures quoted were based on results obtained both in the experiments of Dr Soal and Mr Bateman and in others performed before their arrival.

Ciba Symposium on Extrasensory Perception

From 3-5 May the Ciba Foundation for the Promotion of International Co-operation in Medical and Chemical Research held a symposium in London on Extrasensory Perception. Papers were read by Robert Amadou, G. Spencer Brown, Dr E. J. Dingwall, Dr W. H. Gillespie, J. Langdon-Davies, Dr G. V. T. Matthews, Dr R. A. McConnell, J. Fraser Nicol, Dr M. Pobers, Dr J. G. Pratt, Dr S. G. Soal, Dr G. D. Wassermann, and Dr D. J. West. Others who took part in the Symposium were Dr C. G. Butler, Sir Henry Dale, Professor J. H. Gaddum, Professor A. C. Hardy, Sir Charles Harington, Professor A. J. Lewis, Dr A. S. Parkes, Dr W. L. M. Perry, and J. Salvin. The papers and discussions will be published later in the year by J. & A. Churchill Ltd as a volume in the Ciba Symposia series.

Psychical Research in Argentina

Dr J. Ricardo Musso, one of the founders of the Instituto Argentino de Parapsicologia and author of *En los Limites de la Psicología* (reviewed in the March issue of the *Journal*), tells us that an 'Association of the Friends of Parapsychology' has just been formed in Buenos Aires with the object of promoting critical

investigations (address: Calle Condarco 4801). A quarterly 'Review of Parapsychology', believed to be the first journal of this nature to be published in Spanish, is to be issued under the Association's auspices, with Dr Musso as Editor (address: Cevallos 1766).

A new Italian Journal

Edited by Dr William Mackenzie, to whom psychical research has been a lifelong interest, the first number of *Parapsicologia* has lately appeared. It is published quarterly by Fratelli Bocca Editori, Via Flaminia 133, Rome, and replaces the review *Meta-psichica*.

Fifty Pounds Prize Essay

Members and non-members are reminded that entries must be submitted by 30 June 1955. Details will be found in the *Journal* for November–December 1954 (p. 400) or may be obtained from the Secretary, Society for Psychical Research, 31, Tavistock Square, London, W.C. 1.

